

MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY



I.—OUR DIRECT EXPERIENCE OF TIME

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I

THE problem I discuss in this paper is that of our direct awareness of the passage of events as distinct from our awareness through memory.

The problem arises in the epistemological classics in the form of various answers to the question 'Has an idea duration?' Locke says 'There is a train of ideas which constantly succeed one another in the understanding' (*Essay*, II, xiv, p. 3), and further 'The distance between the appearance of any two ideas in the mind is what we call *duration*' (*ib.*). There is thus, he adds, 'no perception of duration but by considering the train of ideas that take their turns in our understandings' (*ib.*, p. 4). It would seem from these and similar statements that we cannot find the characteristic called 'duration' in a single idea. If so, ideas must be instantaneous. But Locke, as usual, contradicts himself. He says 'The mind cannot fix long on one invariable idea' (*ib.*, p. 13). Here it seems that an idea can endure for a limited but not a long period. Or again 'Every part of duration is duration too . . . capable . . . of division *in infinitum* . . . a small part of duration may be called a moment and is the time of one idea in our mind' (*Essay*, II, xv, p. 9). On this view we can find duration in a single idea.

Hume appears to accept the first of Locke's mutually contradictory propositions. He says 'Time as it exists must be

composed of indivisible moments' (*Treatise*, Bk. I, Part II, ch. ii; ed. Selby Bigge, Oxford, 1896, p. 31), and again 'The idea of duration is always derived from a succession of changeable objects' (*ib.*, ch. iii, p. 37). But if he had applied to time the same kind of analysis that he applied to space he would have found himself left with Locke's second alternative. For the spatial elements into which a perceived colour-expanse must be analysed are coloured points; and by a 'coloured point' Hume makes it clear that he does not mean a mathematical point having position but no magnitude, for such mathematical points can neither exist nor be perceived. He means a very small area, the area in fact of a *minimum visibile* (*Treatise*, Bk. I, Part II, chs. iii and iv; especially p. 42). By similar reasoning he should have concluded that a mathematical instant, having date but no duration, could not exist or be perceived; and that our direct experience of a sound must be analysed into brief sounds, each of them the shortest sound which still remains audible, each having duration.

Reid drove home the second answer. 'There must be duration in every single interval or element of which the whole duration is composed. Nothing indeed is more certain than that every elementary part of duration must have duration' (*Essays on the Intellectual Powers*, Book III, ch. v; ed. A. D. Woozley, p. 209), and again 'we assign to sense not an indivisible point of time but that small portion of time which we call the present, which has a beginning, a middle and an end' (*ib.*, p. 208).

The problem re-emerged in recent epistemology. 'Ideas' vanish. 'Sense-data' reign in their stead. The question now becomes 'How long does a sense-datum last?' If sense-data are to be the elements into which our direct experience of physical objects (perhaps all our experience of physical objects) is to be analysed, Reid's argument would seem to hold. They must last for *some* time, however brief. Russell referred to them as 'lasting only for a very short time' (*Philosophy of Logical Atomism*, II, p. 325).

But there is obviously a difficulty in sorting out these brief particulars from the temporally continuous experience in which they are embedded. Let us recall again the parallel problem concerning space. It has been made clear (by Price, for example) that what is directly given to sight is not a sense-datum but a sense-field. Sense-data are items in sense-fields; they are distinguished from one another or from the rest of the sense-field by content (in the case of sight, by colour). Now this process of discrimination seems to be to some extent arbitrary and in some

cases impossible. When I see a tomato I have a sense-datum which is red and circular and bulgy. But suppose my tomato has a green spot on it. Shall I say I have one sense-datum (red-with-a-green-spot) or two sense-data (a green one completely surrounded by a ring-shaped red one)? It does not matter which I say. Now suppose you are looking at a curtain of shot silk so close to you that it occupies your whole field of view. It is clear that you see colours, in the plural. But it would be impossible to say how many sense-data (distinguished by colour) this sense-field contains. However all this may be, sense-data are distinguished by their content, and Russell and Broad and Price will have no truck with Hume's *minima visibilia*.

Now translate this into terms of time. I lie awake in the dark for half an hour listening to the swishing of the rain. After fifteen minutes the swishing is drowned by the monotone shriek of an owl, lasting three seconds. Shall I say I had one swishing sense-datum half an hour long, interrupted at half-time by a shrieky sense-datum? Or shall I say I had three sense-data: swishy, shrieky, swishy? It does not seem to matter. But suppose the interruption was not an owl but the ululating Air Raid Danger Signal. Now I ask whether I have heard one note or many; and, if many, how many sense-data, distinguishable by content, the siren noise included. No answer is possible. Nevertheless again, despite these difficulties, it might be reasonable to hold that the only way in which a temporally continuous experience can be broken up into elements is by content—i.e. by qualitative differences between the elements. This is implied by a remark of Ayer's. 'A sense content occurs in each of a series of successive sense-fields, each sense-field differing in content from its predecessor. . . . For if there were no difference at all in content there would not be two sense-fields' (*Proc. Aristotelian Society*, xxxiv, p. 54). This means that a sense-field retaining its content unaltered for two minutes would be a single sense-field; what alone differentiates one sense-field from a later one is its content. On this view the duration of any sense-field and therefore of any sense-datum is a purely empirical matter. There is no reason in principle why a sense-datum should not endure for three minutes or three hours. Brief life is here their portion, only because we are busy people or because we are bored with monotony.

There is, however, quite a different kind of answer to the question how long a sense-datum can last. Price says 'It is highly probable that every sense-datum has a finite duration and certain that most have. On the other hand, it is also certain that the duration is at the best very small, probably never more

than a few seconds (it will depend on our span of attention which is notoriously never great).' (*Perception*, p. 115.) Broad agrees. 'A sensible event has a finite duration which may be roughly defined as the time during which it is sensed, as distinct from being remembered . . . what can be sensed at any moment stretches a little way back behind that moment.' (*Scientific Thought*, p. 348.) Broad explicitly and Price implicitly are referring here to the psychological doctrine of the 'Specious Present'. This is not surprising for (of all philosophers who have tackled systematically the problems of perception) these two have shown a pre-eminent knowledge of and interest in psychology. It must be noted that, when Price says 'our span of attention is notoriously never great', he does not mean that we cannot attend to any one subject for long because of fatigue or boredom. He means that the stretch of time which can be apprehended at a given moment by an attentive act of apprehension is not great. This is clear in the last sentence quoted from Broad.

Now philosophers have generally tended to believe that the psychologists have shown experimentally that our direct apprehension of time is restricted to a certain short duration and that they have determined the normal length of this duration by measurement. It is usually given as 0.75 second. The *locus classicus* in English for all this is in William James. He says 'We are constantly aware of a certain duration—the specious present—varying from a few seconds to probably not more than a minute, and this duration (with its content perceived as having one part earlier and another part later) is the original intuition of time' (*Principles of Psychology*, vol. i, p. 642). On this view it is of course impossible that a sense-datum should last two minutes. If I hear a continuous homogeneous sound lasting two minutes, I must be having a series of exactly similar successive sense-data, each of them x seconds long (whatever the length of the specious present is experimentally found to be).

What was the experimental evidence on which the duration of the specious present was first determined? Wundt (1874) and some of his colleagues tried to determine the maximum group of sounds which could be 'remembered as a whole and identified without error'. (I think the truth is that they discovered the group could be identified without error, and inferred that it was therefore remembered as a whole and must accordingly have been heard as a whole.) The maximum duration of such a group was found by Wundt to be six seconds, by Dietze (1885) to be thirty-six seconds. About the same time another group of Wundt's followers tried to discover the time interval which could be esti-

mated with the greatest accuracy. This was found by Kollert (1882) to be 0.75 second and by Mehner (1885) to be 0.71 second. Shorter intervals than these were regularly overestimated, longer intervals regularly underestimated. Estel (1884) and Mehner even found a periodicity by which such 'indifference points' (or maximum accuracy intervals) recurred at regular intervals; according to Estel at multiples of 0.75 second (1.50 seconds, 2.25 seconds, etc.). This led to the general acceptance of 0.75 second as the unit of temporal experience. It does not seem to have troubled people at that time that the two sets of experiments reached such very different results. (We have seen how both are covered by James when he says the specious present 'varies from a few seconds to probably not more than a minute'.)

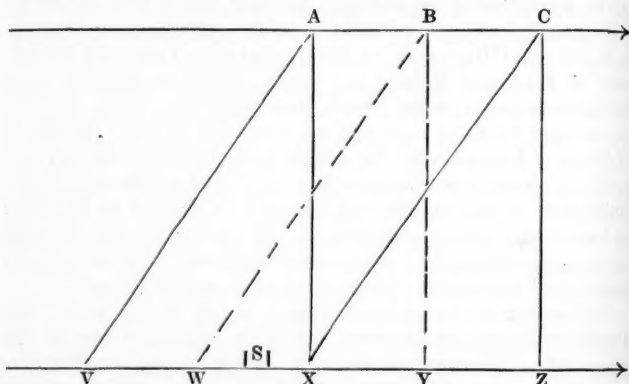
The second set of experiments, however, which gave the result 0.75 second, has been carefully repeated with negative results. L. F. Stevens (MIND, O.S., xi, 1886) failed to find any such periodicity as Estel and Mehner had noted, and he commented that indifference points varied greatly from subject to subject, though the average for those he tested was about 0.6 second. Woodrow (*Journal of Experimental Psychology*, 1930, 1934) showed conclusively that there is nothing absolute about the indifference point. Individual variations were too large for an average to be taken as having any general significance. So we are left with the first set of experiments (the recognition without error of sound groups) which gave a specious present of six or thirty-six seconds.

The notion of the specious present is full of difficulties, and Professor Broad has rendered one of his signal services to the philosophy of science in clarifying it. He gives two presentations of it: in *Scientific Thought* (pp. 348 ff.) and in *An Examination of McTaggart's Philosophy* (Part II, vol. i, pp. 281 ff.). I do not see how any improvement on his analysis is possible, so I shall summarize it briefly with the aid of his diagram.¹ The upper horizontal line represents successive acts of apprehension (A, B, C . . .) following each other in the direction of the arrow. The lower horizontal line represents contents of awareness; sounds, colour-patches etc. A point on the lower line vertically below a point on the upper line is simultaneous with it.

Broad's first point (and the one on which most of the difficulties hinge) is that the definition of the specious present requires the conception of a momentary act of awareness. Let such an act occur at A. Now the doctrine holds that this act A will be aware

¹ Adapted from *Scientific Thought*, p. 349. With the kind permission of the author and his publishers, Messrs. Kegan Paul, Trench, Trubner & Co.

of an event which lasts from V to X (a sound or other datum). This stretch VX is the specious present of act A. It ends at a moment simultaneous with A but includes a short stretch which is past when A is present. A later act of awareness B would be aware of the content WY, and one at C aware of XZ. Now there cannot be momentary acts of awareness just as there cannot be momentary sense-data. Broad raises this difficulty and deals with it with characteristic ingenuity. Let us assume the specious present of an act is six seconds long. If B occurs three seconds after A the specious presents of A and B will overlap (VX overlapping WY). Now A and B cannot represent acts of awareness (since they would be momentary) but AB can. This would be an act of awareness lasting three seconds. What would be



its specious present? What event could this act apprehend as a whole? One is tempted to say 'the event VY'. The answer is 'the event WX'. For the stretch VW cannot be given to act AB as a whole, because at all moments after A it has vanished into the past and is accessible only to memory. And the stretch XY cannot be given to AB as a whole because at all moments before B it is still in the future and therefore not accessible to inspection at all. Now suppose there is an act of awareness lasting six seconds, from A to C. What will be the specious present of that act? What content can be given to it as a whole? The answer is *nil*. For at moment A nothing after X can be experienced because it has not happened yet and at moment C nothing before X can be directly experienced because it has all vanished into the past. Thus we conclude (still supposing that the specious present is six seconds in length) that the specious

present of an act of awareness varies inversely with the duration of the act. As the duration of the act approximates to zero the duration of its specious present approximates to six seconds. As the act approaches six seconds its specious present approximates to zero. All acts of six seconds or more have no specious present. Then, using the Method of Extensive Abstraction, we can define 'the specious present' as the set of all specious presents of actual acts of awareness. They will 'converge' to six seconds. This seems to me a most effective piece of analysis. If the Wundt doctrine does not mean this, I cannot see what it could mean; though no doubt Wundt and his followers had no idea of what was involved.

Broad seems to accept the doctrine, thus explained, as giving an accurate description of our direct experience of time. The effect on me is the exact reverse. It drives me to conclude that the doctrine of the specious present is untenable and sheds no light on our normal apprehension of temporal events.

II

Broad himself draws from his own analysis some corollaries which would have surprised the previous holders of the doctrine. To begin with, he rejects James's 'saddleback' description. 'The present is a saddleback on which we sit perched and from which we look in two directions into time' (*op. cit.*, p. 609). This means that at moment A we apprehend a short stretch of the past and a short stretch of the future. Broad thinks we cannot be directly aware of any future event; in this agreeing with E. R. Clay who originated the term 'specious present'. Clay says 'The present to which the datum refers is really part of the past' ('The Alternative', p. 167 quoted by James, p. 609). Broad also draws attention to a second corollary of his view, though he does not emphasize its paradoxical character. Every actual act of apprehension must have some finite duration. But then no act of apprehension can apprehend any event simultaneous with it. For example, in the diagram, the act which lasts from A to B apprehends as a whole the content WX which has finished occurring at the moment AB begins. As Broad puts it 'The prehended content is completely past at the moment at which it first begins to be prehended' (*Examination of McTaggart*, II, i, p. 288). Thus, if my dentist hurts me, he has always stopped hurting me before I begin to feel the hurt. And this has nothing to do with the time taken by nerve transmission; it is a direct corollary of the specious present theory. Broad thought it an unacceptable paradox of

James's saddleback theory that it involved apprehension of the future. I think it an almost equally unacceptable paradox of Broad's theory that it denies all direct awareness of the present. A third and the most important corollary Broad draws from his analysis is that the specious present cannot be, as James said it was, 'the unit of composition of our perception'. In summing up his view (*op. cit.*, p. 642) James says 'This duration (with its content perceived as having one part earlier and another part later) is the original intuition of time. Longer times are conceived by adding shorter times by dividing portions of this vaguely bounded unit.' Here then is our unit of psychological time; a long experience consists of a number of these units added together. But the diagram shows that this will not do. VX is the specious present of act A and is one unit. The next unit is XZ which is the specious present of act C (six seconds later). If then the *content* of our experience is to be analysed into specious presents as units (each say six seconds long) then our acts of apprehension must take place at intervals of six seconds, intervals during which no apprehension occurs. There could for example be no act of apprehension at B. If, however, there are stretches of continuous awareness ABC then specious presents must overlap each other (VX, WY, XZ) and therefore cannot be the units of which the content of awareness is composed and into which it can be analysed.

If we are to take the doctrine as true and if Broad's analysis is accepted, we must say that a direct experience of a sound lasting say three seconds will be the awareness of an infinite set of overlapping specious presents. Broad accepts this. He says 'No specious present has an immediate successor'. 'The series of specious presents must be compact.' 'Between any two specious presents there will always be an infinite number of others' (*Examination of McTaggart*, II, i, p. 285). I find 'between' an unsatisfactory word to describe the relation between overlapping specious presents—for example 'WY is between VX and XZ'—but it is clear enough what Broad means. I find it hard to believe that this is a description of our experience; but I find it completely convincing that anyone who accepted this doctrine of the specious present should have to describe our experience in this way.

In particular I find the overlapping character puzzling. Take a simple case. Suppose there is a brief sound marked on the diagram as S, I shall experience it at moment A as part of the specious present VX. Three seconds later at moment B I shall experience it again as part of the specious present WY. Every

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brief sound I hear I shall hear not once but repeatedly. Nothing in my direct experience confirms this repetition. If it occurred it would obviously make listening to music or to continuous sentences a matter of the greatest complexity and difficulty.

It is possible, however, to take all this talk of infinite sets and series as an intellectual construction meant to represent, but inevitably falsifying, the continuous character of our experience. To say that our experienced content *consists of* a compact series of overlapping specious presents results from saying that an act of awareness consists of a compact series of instantaneous acts of awareness. But there are no such acts. Our acts of awareness are continuous within their limits and this mathematical analysis of continuity as a compact series of dimensionless points corresponds to nothing in the psychological facts except the continuity. The specious present doctrine might now be restated, with the help of the diagram, thus. Suppose a searchlight mounted on the upper line and shining downwards on the lower line, with a constant span of illumination equal to VX. If you could stop the searchlight at moment A (which you can't) it would illuminate VX. As it moves along the top line X begins to be illuminated at moment A and ceases to be illuminated at moment B. So my objection that I find no repetitions in my experience can be met. When a searchlight moves over a landscape it does not illuminate one object *repeatedly*; it illuminates it *continuously*.

What is wrong with this simile? The answer is that it makes the source of illumination punctiform. As we have seen this would represent a momentary act of apprehension; but these cannot occur. If it is said that the movement of a punctiform source from A to B is again just a mathematical fiction representing the continuity of the act of awareness AB, that again will not do. For it would mean that the content of (= area illuminated by) act AB would be VY whereas (as we have seen) the content apprehended on Broad's view is WX.

In the exposition of the doctrine in previous sections I have followed *Scientific Thought*. In the treatment of the corollaries, I have followed *An Examination of McTaggart* in which they are given much more fully and clearly. The exposition in the later work is more fully developed but I do not find the additions make the theory clearer. *Scientific Thought* lays much more emphasis on the way in which the theory depends on momentary acts of apprehension, as I think it does. The *Examination* seems to substitute for the distinction between act of apprehension and content apprehended a distinction between 'occurring' and

'being prehended' which conceals the problem. It also adds a characteristic of contents which Broad calls 'presentedness' which pervades a specious present, is at its maximum at the part of the specious present most nearly simultaneous with the act of apprehension, and tails off towards zero towards the past. (In my searchlight simile, it is as if the specious present VX had its maximum illumination at X and the degree of illumination diminished leftwards until at V it merges into total darkness.) 'Presentedness' seems to be Broad's name for the characteristic Hume called 'force and vivacity'. But if we are to try to talk this language we shall find 'presentedness' a very odd characteristic. It is (presumably) conferred on the specious present by the act of apprehension (which occurs only after the content has ceased to exist). Moreover there are puzzles about the degree of this characteristic. While the searchlight source remains punctiform the picture of diminution from a maximum (at X) to a minimum (at V) makes sense. But, for any concrete act of awareness, puzzles arise. Take the act AB, whose specious present is WX. Now with what intensity (degree of presentedness) is WX illuminated (presented)? Presumably X is more brightly illuminated than W. But, at moment A, W was illuminated at half strength and, at moment B, at zero strength. At moment A, X is illuminated at full strength, at moment B, at half strength. When the specious present WX is apprehended as a whole by the act AB with what degree of presentedness is it endowed? (This is also another way of showing that the searchlight simile will not work.) So I do not find the later presentation in the *Examination* helpful in making the doctrine easier to understand or to accept. My impression is that when Broad wrote *Scientific Thought* he was satisfied that the specious present theory as worked out by him provided an adequate analysis of the temporal character of direct experience. But in the *Examination*, while he still thinks there is something in the doctrine, he seems less certain of its adequacy as providing such a complete analysis.

III

I now return to the psychological evidence on which the doctrine was founded, in order to raise a number of difficulties about it which are not dealt with by Broad. The aim was to discover the unit of temporal experience. So far as the experiments discovered anything, they found what was the maximum duration of a set of sounds which could be recognized without error. But we must not suppose that we are always apprehending data of

the maximum duration. James sometimes seems to remember this—for instance when he refers, in the passage quoted above, to experiences which vary from a few seconds to not more than a minute. But on other occasions he seems to take the maximum as the norm. 'The specious present has a vaguely vanishing backward and forward fringe but its nucleus is probably the dozen or so seconds which have just elapsed' (*Principles of Psychology*, I, p. 613). Why should not *minima* be taken as the units, as Hume would have implied? (*Minima* vary a good deal with the kind of apparatus used, but results as low as 0.002 second for hearing and 0.04 second for sight have been recorded.)

Secondly, the maxima will vary from individual to individual and with practice and with fatigue. What general conclusions about normal experience and its units can be based on the fact that Jones when fresh and in practice can recognize a group of sounds six (or thirty-six) seconds long?

Thirdly, the experiments rest on one sense only, hearing. It is clear that similar experiments could be performed for sight or touch (with light flashes or taps on the skin). But we have seen that the *minimum* for sound is one-twentieth of the minimum for sight. It seems likely that maxima for different senses would vary as widely.

Fourthly, it is possible to be using two senses at once (at a ballet performance). But then I shall be simultaneously apprehending specious presents of different lengths. And what then is *the* unit? Broad would have a particular difficulty here. For he says 'The special sense-fields of the various senses [sc. of a single subject] form part of a single general sense-field so far as temporal characteristics are concerned. Now *it* is of finite duration' (*Scientific Thought*, p. 360). But *what* duration, if two of the senses which contribute to its content have different specious presents?

Finally, the psychological evidence was based on hearing and was simply rhythmic. It included no variation in pitch. It was inferred that the group of sounds had been heard as a whole because it was recognized without error and without any analytic or symbolic aids, such as counting. But suppose pitch is added to rhythm; then you get a tune, instead of African drums. Now what is the longest tune which can be recognized without error? I should be surprised if the answer was as low as a minute (except for those with no musical ear at all). And I should not be surprised if a trained musician could run that figure up to five minutes or ten. Mozart said he could imagine a complete musical work in his mind. 'Nor do I hear in my imagination the parts successively; I hear them as it were all together' (Holmes, *Life*

of Mozart, p. 317). If this is true it seems likely that he could have recognized without error an item half an hour long.

These points all seem to arise directly from the experimental evidence and to require no special analysis. They all tend to show that this evidence should not have been taken as leading to any general conclusions about the units of normal time experience.

There is a further point which arises if Broad's analysis is accepted. It will be remembered that he defined a specious present in terms of a momentary act of awareness and then got rid of the momentary act by the method of extensive abstraction. But this involved the admission that any *actual* specious present—being the datum of an act which endures a finite time, would be less than the 'defined' specious present. For example if the specious present as defined—i.e. for a momentary act—is six seconds, the specious present of an act of awareness lasting three seconds will be three seconds, that of an act lasting six seconds or more will be *nil*. My problem is this. It is clear that the empirical evidence was not based on momentary acts of awareness (for there are none). It is equally clear that no attempt was made to measure the duration of the acts of awareness, as well as measuring the duration of the sound groups apprehended. Yet without such additional measurement no definite results could be attained. But how could the duration of the act of attention have been measured? Suppose I listen to a drum-tap group and then listen to a second drum-tap group and identify it without error as having the same number of taps as the first. The question is this. How long did the act of awareness take which apprehended either group? Surely the most natural answer is that the act took as long as the group. But if the group is a maximum, we then have a contradiction; for the specious present of an act as long as the maximum is *nil*.

IV

It is interesting and, I think, significant that psychologists, ever since the original work of Wundt and his followers have tended to neglect the specious present. Woodrow's experiments which refuted the indifference point theory are the only ones of which I know in this field since 1890. Was this due to the fact that the notion had not been so clarified as to be a possible basis for future experiment? Or was it perhaps that the notion cannot be so clarified?

The only psychologist who has given consecutive and serious

attention to the subject appears to be E. G. Boring. (*The Physical Dimensions of Consciousness* (1933), pp. 127 ff. *Temporal Perception and Operationism* (*American Journal of Psychology*, vol. xlviii (1936), p. 591). *Sensation and Perception in the History of Experimental Psychology*, pp. 575 ff. I owe these references to Mr. B. A. Farrell.) I was interested to find how considerably Boring agreed with the conclusions I had already reached. In the first of these works Boring accepted the doctrine of the specious present and tried to make sense of it. But he makes two points. First, he is doubtful whether the specious present should be described as immediately or directly given since it involves an integration or principle of organization (the *rhythm* in the experiments of Wundt and Dietze) which can 'pick unities out of the continuous stream of time'. Secondly, he notices the difficulty of the 'act of apprehension'. 'If a duration is immediately observed, *when* do you observe it? . . . It is a foolish question. Observation is a process. It is not instantaneous and therefore cannot be confined to a single moment. . . . In the case under consideration, the observation in a way begins with the duration in question and it ends a little way after it, when the integration has fulfilled itself observationally. . . . It is just as true to say the observation is the end phase of the integration and therefore comes after it. In either case it is a process' (*op. cit.*, p. 137). In the article in the *American Journal of Psychology*, Boring develops his attack on 'immediacy', and links the specious present with this attack. Finally, in his later book (which includes a full account of the work on the specious present and a bibliography) he completely gives up the specious present doctrine. He points out that the doctrine was the outcome of the analytic method current in Wundt's time. 'What these men in Wundt's laboratory were hoping to find was a psychological unit of time. . . . Munsterberg was nearer the truth in 1889. He said that it was futile to look for fixed units of mental duration' (*op. cit.*, p. 580). Boring thinks this whole approach was mistaken. 'In modern positivistic psychology the problem has disappeared' (p. 577).

V

What are we to conclude? Our experience of temporal process is continuous (except during periods of total unconsciousness). Any sub-divisions within it can be made only in terms of content. Such sub-division is always to some extent arbitrary and sometimes impossible. (Arbitrary in the case of a *glissando* on the piano—a two-second *glissando* noise or thirty-seven brief noises;

impossible in the case of a *glissando* on the violin—one noise or many, and if many how many?) But what then of the empirical evidence on which the specious present originally rested? We need not deny that groups of sounds can be recognized and identified without error. But this seems to be just a piece of empirical information which throws no light at all on the normal and invariable characteristics of our experience of time. In just the same way, the experiments on *minima* give us empirical information about certain time experiences. But we should not go on to conclude that they have any bearing on our normal experience—for example that my awareness of a sound consists of a series of sounds each 0.002 second long.

The specious present theory was right in holding that it is impossible to be aware of an event with no duration at all. But Reid had insisted on this long before Wundt's time. Any attempt to discover the duration of the unit-events which make up the temporal process of normal experience is doomed to failure.

The recognition of sound groups is a particular case of 'gestalt' apprehension. The experimentalists found that a group of sixteen sounds was more easily recognized if it were given in a pattern of four fours or eight pairs than if given evenly or in random sub-groups. Gestalt psychologists themselves do not appear to have emphasized the gestalt character as defining the units of our experience of time. An example of such a definition is, however, given by M. A. Tinker (*Psychology*, edited by Boring, Langfeld and Weld, 1935, p. 247). 'The conscious present is the length of time that an integrated whole—a musical phrase or a joke—takes; it is the duration of any series of events that depends on its temporal completeness for its proper psychological functioning.' This goes further than Tinker supposes. A Bach fugue depends on its temporal completeness for its proper psychological functioning and it may last fifteen minutes. So does one of the démodé short stories of O. Henry with the point in the tail. And there are longer jokes still—for example George Orwell's *Animal Farm*. Koffka himself deals with the units of time-perception in chapter xv of the *Principles of Gestalt Psychology*. He says 'The present or the unit for time perception is determined by homogeneity of stimulation'. This, I take it, is the physiological equivalent of my view that qualitative differences of content alone determine sub-divisions within our continuous experience. But Koffka also gives examples to show how the past determines the present, for example how the perceived character of a note depends on its setting in a phrase or tune. Here the isolated note cannot be taken as the unit.

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To sum up. I have been arguing that the whole conception of units of temporal experience is mistaken; that this conception provided the stimulus for the experiments on which the specious present theory was founded; that the work on the specious present theory (while providing interesting empirical data about certain gestalt time experiences) does not justify any conclusions about time experience in general; and that the specious present doctrine as described and defended by psychologists and as rationalized and clarified by Broad is an untenable hypothesis. These conclusions are of course mainly negative. I have a further negative conclusion which I add more tentatively. This paper is called 'Our Direct Experience of Time'. In the course of my analysis of the specious present theory I have become (as Boring did for very different reasons) steadily more dubious about the word 'direct'. Owl cries and swishings of rain might be said to be directly experienced. But then follow gestalt cases where the unity depends on a form or pattern, which might be repeated in different keys and still recognized without any awareness of the change of key. Or they might be repeated in a different medium altogether, buzzes instead of taps, and recognized again. Then there were musical phrases and tunes; and then again there are jokes. All the time we are moving further from the notion of direct or immediate experience, if this means absence of organization, of structure, of intelligible relations. Immediate experience, pure sensation, here as elsewhere seems to be not a ubiquitous datum, not a necessary ingredient, not a basic constituent of our experience, to be revealed there by careful analysis. It seems to be more like a limiting case to which experience sometimes approximates, but into which it would be dangerous to import those discriminable time factors discoverable in developed experience. But these more tentative conclusions go beyond the intentions of the present paper, which is meant primarily to show that the specious present is 'specious' in a sense not intended by its supporters.

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II.—ON CONCEPT AND OBJECT

BY GOTTLOB FREGE

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Translation by P. T. GEACH. (Revised by MAX BLACK.)

192] In a series of articles in this Quarterly on intuition and its psychical elaboration, Benno Kerry has several times referred to my *Grundlagen der Arithmetik* and other works of mine, sometimes agreeing and sometimes disagreeing with me. I cannot but be pleased at this, and I think the best way I can show my appreciation is to take up the discussion of the points he contests. This seems to me all the more necessary, because his opposition is at least partly based on a misunderstanding, which might be shared by others, of what I say about the concept; and because, even apart from this special occasion, the matter is important and difficult enough for a more thorough treatment than seemed to me suitable in my *Grundlagen*.

The word 'concept' is used in various ways; its sense is sometimes psychological, sometimes logical, and sometimes perhaps a confused mixture of both. Since this licence exists, it is natural to restrict it by requiring that when once a usage is adopted it shall be maintained. What I decided was to keep strictly to a purely logical use; the question whether this or that use is more appropriate is one that I should like to leave on one side, as of minor importance. Agreement about the mode of expression will easily be reached when once it is recognized that there is something that deserves a special term.

It seems to me that Kerry's misunderstanding results from his unintentionally confusing his own usage of the word 'concept' with mine. This readily gives rise to contradictions, for which my usage is not to blame.

193] Kerry contests what he calls my definition of 'concept'. I would remark, in the first place, that my explanation is not meant as a proper definition. One cannot require that everything shall be defined, any more than one can require that a chemist shall decompose every substance. What is simple cannot be decomposed, and what is logically simple cannot have a proper definition. Now something logically simple is no more given us

at the outset than most of the chemical elements are ; it is reached only by means of scientific work. If something has been discovered that is simple, or at least must count as simple for the time being, we shall have to coin a term for it, since language will not originally contain an expression that exactly answers. On the introduction of a name for something logically simple, a definition is not possible ; there is nothing for it but to lead the reader or hearer, by means of hints, to understand the words as is intended.

Kerry would like to say that the distinction between concept and object is not absolute. "In a previous passage", he says, "I have myself expressed the opinion that the relation between the content of the concept and the concept-object is, in a certain respect, a peculiar and irreducible one ; but this was in no way bound up with the view that the properties of being a concept and of being an object are mutually exclusive. The latter view no more follows from the former than it would follow, if, *e.g.*, the relation of father and son were one that could not be further reduced, that a man could not be at once a father and a son (though of course not *e.g.* father of the man whose son he was)."

Let us fasten on this simile ! If there were, or had been, beings that were fathers but could not be sons, such beings would obviously be quite different in kind from all men, who are sons. Now it is something like this that happens here. The concept (as I understand the word) is predicative.¹ On the other hand, a name of an object, a proper name, is quite incapable of being used as a grammatical predicate. This admittedly needs elucidation, otherwise it might appear false. Surely one can just as well assert of a thing that it is Alexander the Great, or is the number four, or is the planet Venus, as that it is green or is a mammal ? [194] If anybody thinks this, he is not distinguishing the usages of the word 'is'. In the last two examples it serves as a copula, as a mere verbal sign of predication. (In this sense [the German word *ist*] can sometimes be replaced by the mere personal suffix : *cf. dies Blatt ist grün* and *dies Blatt grünt.*) In such a case we say that something falls under a concept, and the grammatical predicate stands for this concept. In the first three examples, on the other hand, 'is' is used like the 'equals' sign in arithmetic, to express an equation.² In the sentence 'The morning star is

¹ It is, in fact, the reference of a grammatical predicate.

² I use the word 'equal' and the symbol '=' in the sense 'the same as', 'no other than', 'identical with'. *Cf.* E. Schroeder, *Vorlesungen ueber die Algebra der Logik* (Leipzig, 1890), vol. 1, §1. Schroeder must however be criticized for not distinguishing two fundamentally different

Venus', we have two proper names, 'morning star' and 'Venus', for the same object. In the sentence 'the morning star is a planet' we have a proper name, 'the morning star', and a concept-word, 'planet'. So far as language goes, no more has happened than that 'Venus' has been replaced by 'a planet'; but really the relation has become wholly different. An equation is reversible; an object's falling under a concept is an irreversible relation. In the sentence 'the morning star is Venus', 'is' is obviously not the mere copula; its content is an essential part of the predicate, so that the word 'Venus' does not constitute the whole of the predicate.¹ One might say instead: 'the morning star is no other than Venus'; what was previously implicit in the single word 'is' is here set forth in four separate words, and in 'is no other than' the word 'is' now really is the mere copula. What is predicated here is thus not *Venus* but *no other than Venus*. These words stand for a concept; admittedly only one object falls under this, but such a concept must still always be distinguished from the object.² We have here a word 'Venus' that can never be a proper predicate, although it can [195] form part of a predicate. The reference³ of this word is thus something that can never occur as a concept, but only as an object. Kerry too would probably not wish to dispute that there is something of this kind. But this would mean admitting a distinction, which it is very important to recognize, between what can occur only as an object, and everything else. And this distinction would not be effaced even if it were true, as Kerry thinks it is, that there are concepts that can also be objects.

There are, indeed, cases that seem to support his view. I myself have indicated (in *Grundlagen*, §53, *ad fin.*) that a concept may fall under a higher concept—which, however, must not be confused with one concept's being subordinate to another. Kerry does not appeal to this; instead, he gives the following example: "the concept 'horse' is a concept easily attained", and thinks that the concept 'horse' is an object, in fact one of the objects that fall under the concept 'concept easily attained'. Quite so; the three words "the concept 'horse'" do designate an object, but on that very account they do not designate a concept, as I

relations; the relation of an object to a concept it falls under, and the subordination of one concept to another. His remarks on the *Vollwurzel* are likewise open to objection. Schroeder's symbol \neq does not simply take the place of the copula.

¹ Cf. my *Grundlagen*, §66, footnote.

² *Ibid.*, §51.

³ Cf. my paper 'On Sense and Reference' (*Ueber Sinn und Bedeutung*), shortly to appear in the *Zeitschrift für Phil. und phil. Kritik*.

am using the word. This is in full accord with the criterion I gave—that the singular definite article always indicates an object, whereas the indefinite article accompanies a concept-word.¹

Kerry holds that no logical rules can be based on linguistic distinctions; but my own way of doing this is something that nobody can avoid who lays down such rules at all; for we cannot come to an understanding with one another apart from language, and so in the end we must always rely on other people's understanding words, inflexions, and sentence-construction in essentially the same way as ourselves. As I said before, I was not trying to give a definition, but only hints; and to this end I appealed to the general feeling for the German language. It is here very much to my advantage that there is such good accord between the linguistic distinction and the real one. As regards the indefinite article there are probably no exceptions to our rule at all for us to remark, apart from obsolete formulas like 'Ein edler Rath' ['Councillor']. The matter is not so simple for the definite article, especially in the plural; but

196] then my criterion does not relate to this case. In the singular, so far as I can see, the matter is doubtful only when a singular takes the place of a plural, as in the sentence 'the Turk besieged Vienna', 'the horse is a four-legged animal'. These cases are so easily recognizable as special ones that the value of our rule is hardly impaired by their occurrence. It is clear that in the first sentence 'the Turk' is the proper name of a people. The second sentence is probably best regarded as expressing a universal judgment, say 'all horses are four-legged animals' or 'all properly constituted horses are four-legged animals'; these will be discussed later.² Kerry calls my criterion unsuitable;

¹ *Grundlagen*, §51; §66, footnote; §68, footnote on p. 80.

² Nowadays people seem inclined to exaggerate the scope of the statement that different linguistic expressions are never completely equivalent, that a word can never be exactly translated into another language. One might perhaps go even further, and say that the same word is never taken in quite the same way even by men who share a language. I will not enquire as to the measure of truth in these statements; I would only emphasize that nevertheless different expressions quite often have something in common, which I call the sense, or, in the special case of sentences, the thought. In other words; we must not fail to recognize that the same sense, the same thought, may be variously expressed; thus the difference does not here concern the sense, but only the apprehension, shading, or colouring of the thought, and is irrelevant for logic. It is possible for one sentence to give no more and no less information than another; and, for all the multiplicity of languages, mankind has a common stock of thoughts. If all transformation of the expression were forbidden on the plea that this would alter the content as well, logic would simply be crippled; for the

for surely, he says, in the sentence 'the concept that I am now talking about is an individual concept' the name composed of the first eight words stands for a concept; but he is not taking the word 'concept' in my sense, and it is not in what I have laid down that the contradiction lies. But nobody can require that my mode of expression shall agree with Kerry's.

It must indeed be recognized that here we are confronted by an awkwardness of language, which I admit cannot be avoided, if we say that the concept *horse* is not a concept,¹ whereas, e.g., the 197] city of Berlin is a city and the volcano Vesuvius is a volcano. Language is here in a predicament that justifies the departure from custom. The peculiarity of our case is indicated by Kerry himself, by means of the quotation-marks around 'horse'; I use italics to the same end. There was no reason to mark out the words 'Berlin' and 'Vesuvius' in a similar way. In logical discussions one quite often needs to assert something about a concept, and to express this in the form usual for such assertions, viz., to make what is asserted of the concept into the content of the grammatical predicate. Consequently, one would expect that the reference of the grammatical subject would be the concept; but the concept as such cannot play this part, in view of its predicative nature; it must first be converted into an object,² or, speaking more precisely, represented by an object. We designate this object by prefixing the words 'the concept'; e.g.

'The concept *man* is not empty'.

Here the first three words are to be regarded as a proper name,³ which can no more be used predicatively than 'Berlin' or 'Vesuvius'. When we say 'Jesus falls under the concept *man*', then, setting aside the copula, the predicate is:

'someone falling under the concept *man*'

and this means the same as:

'a man'.

task of logic can hardly be performed without trying to recognize the thought in its manifold guises. Moreover, all definitions would then have to be rejected as false.

¹ A similar thing happens when we say as regards the sentence 'this rose is red': the grammatical predicate 'is red' belongs to the subject 'this rose'. Here the words "The grammatical predicate 'is red'" are not a grammatical predicate but a subject. By the very act of explicitly calling it a predicate, we deprive it of this property.

² Cf. my *Grundlagen*, p. X.

³ I call anything a proper name if it is a sign for an object.

But the phrase

'the concept *man*'

is only part of this predicate.

Somebody might urge, as against the predicative nature of the concept, that nevertheless we speak of a subject-concept. But even in such cases, *e.g.*, in the sentence

'all mammals have red blood'

we cannot fail to recognize the predicative nature¹ of the concept; for we could say instead:

198] 'whatever is a mammal has red blood'

or: 'if anything is a mammal, then it has red blood'.

When I wrote my *Grundlagen der Arithmetik*, I had not yet made the distinction between sense and reference;² and so, under the expression 'content of a possible judgment', I was combining what I now designate by the distinctive words 'thought' and 'truth-value'. Consequently, I no longer entirely approve of the explanation I then gave (*op. cit.*, p. 77), as regards its wording; my view is, however, still essentially the same. We may say in brief, taking 'subject' and 'predicate' in the linguistic sense: A concept is the reference of a predicate; An object is something that can never be the whole reference of a predicate, but can be the reference of a subject. It must here be remarked that the words 'all', 'any', 'no', 'some', are prefixed to concept-words. In universal and particular affirmative and negative sentences, we are expressing relations between concepts; we use these words to indicate the special kind of relation. They are thus, logically speaking, not to be more closely associated with the concept-words that follow them, but are to be related to the sentence as a whole. It is easy to see this in the case of negation. If in the sentence

'all mammals are land-dwellers'

the phrase 'all mammals' expressed the logical subject of the predicate *are land-dwellers*, then in order to negate the whole

¹ What I call here the predicative nature of the concept is just a special case of the need of supplementation, the 'unsaturatedness', that I gave as the essential feature of a function in my work *Funktion und Begriff* (Jena, 1891). It was there scarcely possible to avoid the expression 'the function $F(x)$ ', although there too the difficulty arose that the reference of this expression is not a function.

² Cf. my essay 'Sense and Reference' in the *Zeitschrift für Phil. und phil. Kritik*.

sentence we should have to negate the predicate : 'are not land-dwellers'. Instead, we must put the 'not' in front of 'all'; from which it follows that 'all' logically belongs with the predicate. On the other hand, we do negate the sentence 'The concept *mammal* is subordinate to the concept *land-dweller*' by negating the predicate : 'is not subordinate to the concept *land-dweller*'.

If we keep it in mind that in my way of speaking expressions like 'the concept *F*' designate not concepts but objects, most of Kerry's objections

199] already collapse. If he thinks (*cf.* p. 281) that I have identified concept and extension of concept, he is mistaken; I merely expressed my view that in the expression 'the number that applies to the concept *F* is the extension of the concept *equinumerous to the concept F*' the words 'extension of the concept' could be replaced by 'concept'. Notice carefully that here the word 'concept' is combined with the definite article. Besides this was only a casual remark; I did not base anything upon it.

Thus Kerry does not succeed in filling the gap between concept and object. Someone might attempt, however, to make use of my own statements in this sense. I have said that to assign a number involves an assertion about a concept;¹ I speak of properties asserted of a concept, and I allow that a concept may fall under a higher one.² I have called existence a property of a concept. How I mean this to be taken is best made clear by an example. In the sentence 'there is at least one square root of 4', we have an assertion, not about (say) the definite number 2, nor about -2, but about a concept, *square root of 4*; *viz.*, that it is not empty. But if I express the same thought thus : 'The concept *square root of 4* is realized', then the first six words form the proper name of an object, and it is about this object that something is asserted. But notice carefully that what is asserted here is not the same thing as was asserted about the concept. This will be surprising only to somebody who fails to see that a thought can be split up in many ways, so that now one thing, now another, appears as subject or predicate. The thought itself does not yet determine what is to be regarded as the subject. If we say 'the subject of this judgment', we do not designate anything definite unless at the same time we indicate a definite kind of analysis; as a rule, we do this in connexion with a definite wording. But we must never forget that different sentences may express the

¹ *Grundlagen*, §46.

² *Ibid.*, §53.

same thought. For example, the thought we are considering could also be taken as an assertion about the number 4 :

'the number 4 has the property that there is something of which it is the square'.

Language has means of presenting now one, now another, part [200] of the thought as the subject ; one of the most familiar is the distinction of active and passive forms. It is thus not impossible that one way of analysing a given thought should make it appear as a singular judgment ; another, as a particular judgment ; and a third, as a universal judgment. It need not then surprise us that the same sentence may be conceived as an assertion about a concept and also as an assertion about an object ; only we must observe that what is asserted is different. In the sentence 'there is at least one square root of 4' it is impossible to replace the words 'square root of 4' by 'the concept *square root of 4*' ; that is, the assertion that suits the concept does not suit the object. Although our sentence does not present the concept as a subject, it asserts something about it ; it can be regarded as expressing the fact that a concept falls under a higher one.¹ But this does not in any way efface the distinction between object and concept. We see to begin with that in the sentence 'there is at least one square root of 4' the predicative nature of the concept is not belied ; we could say 'there is something that has the property of giving the result 4 when multiplied by itself'. Hence what is here asserted about a concept can never be asserted about an object ; for a proper name can never be a predicative expression, though it can be part of one. I do not want to say it is false to assert about an object what is asserted here about a concept ; I want to say it is impossible, senseless, to do so. The sentence 'there is Julius Cæsar' is neither true nor false but senseless ; the sentence 'there is a man whose name is Julius Cæsar' has a sense, but here again we have a concept, as the indefinite article shows. We get the same thing in the sentence 'there is only one Vienna'. We must not let ourselves be deceived because language often uses the same word now as a proper name, now as a concept-word ; in our example, the numeral indicates that we have the latter ; 'Vienna' is here a concept-word, like 'metropolis'. Using it in this sense, we may say : 'Trieste is no Vienna'. If, on the other hand, we substitute 'Julius Cæsar' [201] for the proper name formed by the first six words of the

¹ In my *Grundlagen* I called such a concept a second-order concept ; in my work *Funktion und Begriff* I called it a second-level concept, as I shall do here.

sentence 'the concept *square root of 4* is realized', we get a sentence that has a sense but is false; for the assertion that something is realized (as the word is being taken here) can be truly made only about a quite special kind of objects, viz., such as can be designated by proper names of the form 'the concept *F*'. Thus the words 'the concept *square root of 4*' have an essentially different behaviour, as regards possible substitutions, from the words 'square root of 4' in our original sentence; that is, the reference of the two phrases is essentially different.¹

What has been shown here in one example holds good generally; the behaviour of the concept is essentially predicative, even where something is being asserted about it; consequently it can be replaced there only by another concept, never by an object. Thus the assertion that is made about a concept does not suit an object. Second-level concepts, which concepts fall under, are essentially different from first-level concepts, which objects fall under. The relation of an object to a first-level concept that it falls under is different from the (admittedly similar) relation of a first-level to a second-level concept. (To do justice at once to the distinction and to the similarity, we might perhaps say: An object falls *under* a first-level concept; a concept falls *within* a second-level concept.) The distinction of concept and object thus still holds, with all its sharpness.²

With this there hangs together what I have said (*Grundlagen*, §53) about my usage of the words 'property' and 'mark'; Kerry's discussion gives me occasion to revert once more to this. The words serve to signify relations, in sentences like ' Φ is a property of Γ ' and ' Φ is a mark of Ω '. In my way of speaking, a thing can be at once a property and a mark, but not of the same thing. I call the concept under which an object falls its properties; thus

'to be Φ is a property of Γ '

¹ Cf. my essay 'Sense and Reference' (cited above).

² [When Russell says that expressions like 'the King of France' are not names but incomplete symbols, he is saying what would be put thus in Frege's terminology: "In 'the King of France is bald', 'the King of France' is not a name of an object; what it stands for is something incomplete, *ungesättigt*—a second-level concept, within which the concept *bald* is falsely asserted to fall. The second-level concept in question is the concept: *concept under which somebody falls who is a King of France and apart from whom nobody is a King of France*; no first-level concept falls within this, because nobody is a King of France."

It should, however, be emphasized that Frege himself gives an entirely different account of definite descriptions. Cf. *Ueber Sinn und Bedeutung*, pp. 39-42.—P.T.G.]

is just another way of saying :

' Γ falls under the concept of a Φ '.

If the object Γ has the properties Φ , X , and Ψ , I may combine them into Ω ; so that it is the same thing if I say that Γ has the property Ω , or, that Γ

202] has the properties Φ , X , and Ψ . I then call Φ , X , and Ψ marks of the concept Ω , and, at the same time, properties of Γ . It is clear that the relations of Φ to Γ and to Ω are quite different, and that consequently different terms are required. Γ falls under the concept Φ ; but Ω , which is itself a concept, cannot fall under the first-level concept Φ ; only to a second-level concept could it stand in a similar relation. Ω is, on the other hand, subordinate to Φ .

Let us consider an example, Instead of saying :

'2 is a positive number' and

'2 is a whole number' and

'2 is less than 10'

we may also say

'2 is a positive whole number less than 10'.

Here

to be a positive number,

to be a whole number,

to be less than 10,

appear as properties of the object 2, and also as marks of the concept

positive whole number less than 10.

This is neither positive, nor a whole number, nor less than 10. It is indeed subordinate to the concept *whole number*, but does not fall under it.

Let us now compare with this what Kerry says in his second article (p. 224). "By the number 4 we understand the result of additively combining 3 and 1. The concept object here occurring is the numerical individual 4 ; a quite definite number in the natural number-series. This object obviously bears just the marks that are named in its concept, and no others besides—provided we refrain, as we surely must, from counting as *propria* of the object its infinitely numerous relations to all other individual numbers ; ('the' number 4 is likewise the result of additively combining 3 and 1.)"

We see at once that my distinction between property and mark is here quite slurred over. Kerry distinguishes here between the number 4 and 'the' number 4. I must confess that this distinction is incomprehensible to me. The number 4 is to be a concept; 'the' number 4 is to be a concept-object, and none other than the numerical individual 4. It needs no

203] proof that what we have here is not my distinction between concept and object. It almost looks as though what was floating (though very obscurely) before Kerry's mind were my distinction between the sense and the reference of the words 'the number 4'. But it is only the reference of the words that can be said to be the result of additively combining 3 and 1.

Again, how are we to take the word 'is' in the sentences 'the number 4 is the result of additively combining 3 and 1' and "'the' number 4 is the result of additively combining 3 and 1"? Is it a mere copula, or does it help to express a logical equation? In the first case, 'the' would have to be left out before 'result', and the sentences would go like this :

'The number 4 is a result of additively combining 3 and 1';
 "'The' number 4 is a result of additively combining 3 and 1."

In that case, the objects that Kerry designates by

'the number 4' and "'the' number 4"

would both fall under the concept

result of additively combining 3 and 1.

And then the only question would be what difference there was between these objects. (I am here using the words 'object' and 'concept' in my accustomed way.) I should express as follows what Kerry is apparently trying to say :—

'The number 4 has those properties, and those alone, which are marks of the concept : *result of additively combining 3 and 1.*'

I should then express as follows the sense of the first of our two sentences :

'To be a number 4 is the same as being a result of additive combination of 3 and 1';

In that case, what I conjectured just now to have been Kerry's intention could also be put thus :—

'The number 4 has those properties, and those alone, which are marks of the concept *a number 4*'.

(We need not here decide whether this is true.)

204] The inverted commas around the definite article in the words " ' the ' number 4 " could in that case be omitted.

But in these attempted interpretations we have assumed that in at least one of the two sentences the definite articles in front of ' result ' and ' number 4 ' were inserted only by an oversight. If we take the words as they stand, we can only regard them as having the sense of a logical equation, like :

' The number 4 is none other than the result of additively combining 3 and 1 '.

The definite article in front of ' result ' is here logically justified only if it is known (i) that there is such a result (ii) that there is not more than one. In that case, the phrase designates an object, and is to be regarded as a proper name. If both of our sentences were to be regarded as logical equations, then, since their right sides are identical, it would follow from them that the number 4 is ' the ' number 4, or, if you prefer, that the number 4 is no other than ' the ' number 4 ; and so Kerry's distinction would have been proved untenable. However, it is not my present task to point out contradictions in his exposition ; his way of taking the words ' object ' and ' concept ' is not properly my concern here. I am only trying to set my own usage of these words in a clearer light, and incidentally show that in any case it differs from his, whether that is consistent or not.

I do not at all dispute Kerry's right to use the words ' concept ' and ' object ' in his own way, if only he would respect my equal right, and admit that with my use of terms I have got hold of a distinction of the highest importance. I admit that there is a quite peculiar obstacle in the way of an understanding with my reader. By a kind of necessity of language, my expressions, taken literally, sometimes miss my thought ; I mention an object, when what I intend is a concept. I fully realize that in such cases I was relying upon a reader who would be ready to meet me half-way—who does not begrudge a pinch of salt.

Somebody may think that this is an artificially created difficulty ; that there is no need at all to take account of such an unmanageable thing as what I call a concept ; that one might, like Kerry, regard an object's falling under a concept as a relation, in which the same thing could occur now as object, now as concept. 205] The words ' object ' and ' concept ' would then serve only to indicate the different positions in the relation. This may be done ; but anybody who thinks the difficulty is avoided this way is very much mistaken ; it is only shifted. For not all the parts of a thought can be complete ; at least one must be ' unsaturated ' ,

or predicative; otherwise they would not hold together. For example, the sense of the phrase 'the number 2' does not hold together with that of the expression 'the concept *prime number*' without a link. We apply such a link in the sentence 'the number 2 falls under the concept *prime number*'; it is contained in the words 'falls under', which need to be completed in two ways—by a subject and an accusative; and only because their sense is thus 'unsaturated' are they capable of serving as a link. Only when they have been supplemented in this twofold respect do we get a complete sense, a thought. I say that such words or phrases stand for a relation. We now get the same difficulty for the relation that we were trying to avoid for the concept. For the words 'the relation of an object to the concept it falls under' designate not a relation but an object; and the three proper names 'the number 2', 'the concept *prime number*', 'the relation of an object to a concept it falls under', hold aloof from one another just as much as the first two do by themselves; however we put them together, we get no sentence. It is thus easy for us to see that the difficulty arising from the 'unsaturatedness' of one part of the thought can indeed be shifted, but not avoided. 'Complete' and 'unsaturated' are of course only figures of speech; but all that I wish or am able to do here is to give hints.

It may make it easier to come to an understanding if the reader compares my work *Funktion und Begriff*. For over the question what it is that is called a function in Analysis, we come up against the same obstacle; and on thorough investigation it will be found that the obstacle is essential, and founded on the nature of our language; that we cannot avoid a certain inappropriateness of linguistic expression; and that there is nothing for it but to realize this and always take it into account.

III.—DECISIONS AND DESCRIPTIONS

BY K. BAIER

THE difference between moral and scientific discourse is often claimed to be that between decisions and descriptions. Scientific discourse is supposed to be descriptive, moral discourse prescriptive; a scientific utterance to be a description, a moral utterance a decision, or what is taken to be equivalent, a choice, a prescription, a norm. As far as I can make out, none of the philosophers using these terms state clearly what they mean by 'decision' and by 'description'. It is, therefore, not unfair to assume that readers will understand these key terms in their ordinary meaning. In this paper, I explain this ordinary meaning. It then becomes apparent how very misleading is the view that moral utterances are not descriptions, but decisions.

I

Before entering on the main task of this paper, the examination of the proper use of 'decision' and 'description', I must say a few words about the use of verbs in general. It is a widely held opinion that every verb is the name of, or stands for an activity. But obviously verbs like 'to know', 'to intend', 'to forget', etc., are not the names of activities. The error involved is twofold: for on the one hand, what we may use these verbs to refer to is not always properly called an activity, and on the other, the use of some verbs is very much more complex than that use of a word which we call a name.

If we want to understand the meaning of verbs such as 'assert', 'command', 'decide', 'describe', etc., it is not enough to examine the forms of words we use in asserting, commanding, etc. Roughly, we must consider the presuppositions, the implications, the occasion, the aim, the proper linguistic form if any, and the logical consequences of someone actually doing one of these things or claiming that someone is doing them. Let me enlarge on some of the more important points.

Every use of language should obviously accord with the rules of grammar. Otherwise it might be *talking incorrectly*, perhaps even *talking nonsense* or *babbling*. In order to be properly referred to by a certain term (e.g. 'assertion', 'command', etc.),

some actions or activities require the choice of the right form and kind of sentence ("When did you come home?" cannot properly be called 'an assertion'), some the right sort of words or formulae ("Here is your Doctor!" cannot be the *conferring of the Doctorate of Music*), performance by the right sort of person (A schoolteacher cannot *grant the Decree Nisi*), on the right occasion (The absent-minded Professor, bending over the baby in the font muttering "I christen this ship *Hindustani*" is not *christening a ship*), in the right tone of voice ("Welcome Home!" said in a menacing tone is not *welcoming anyone home*). Some require the fulfilment of certain presuppositions ("Jones has stopped beating his wife"), others the successful completion of an activity ("She persuaded him to come home"). Some require that the agent should have a certain purpose or aim ("He copied the Vermeer"), some that a certain mental condition is fulfilled ("He disobeyed Jones' order").

Of course, it is not the case that we would never say of someone that he had done something or other unless all these conditions were fulfilled. But if an action or activity requires the fulfilment of one or other of them, then the contention that someone is doing the thing in question is false and must be withdrawn if the condition is not fulfilled.

In addition, if Jones properly and correctly says that Roberts is doing something, then sometimes certain logical consequences follow. I discuss in detail two sorts of consequences. The first sort consists of propositions about someone's state of mind, the second of propositions about, roughly, what someone is committed to.

In "Roberts said that Jones was at home" the implication is that Roberts believed that Jones was at home. That is to say, for anyone believing that Roberts said that Jones was at home it will be wrong to assume that Roberts did not believe that Jones was at home, unless he has reason to distrust Roberts. In other words, there is a rule about what Roberts must be taken to believe when Roberts says something. If this turns out to be false, *i.e.* if Roberts was dishonest, it nevertheless remains true that Roberts said Jones was at home. Nor need it be untrue that Jones is at home. But Roberts may rightly be *reproached* for having said something which he himself did not believe.

If Smith had said "Roberts asserted that Jones was at home", the implication would have been that Smith doubted the truth and/or sincerity of Roberts' remark. Hence it would be odd for Roberts to say "I assert that Jones is at home". For a similar reason it would be odd (but more so) to say "Jones is at home but

I don't think so". For the same reason it is quite natural, though somewhat pleonastic, to speak of ill-founded or unreliable assertions, but quite unnatural to speak of well-founded or reliable ones. In "Roberts assured me that Jones was at home", on the other hand, the implication is that Smith believes that Roberts is sincere and that he, Smith, has no reason to think Roberts is in error. Thus, contrary to a popular view, 'asserting something' does not mean the same as 'expressing one's views about something' or 'contending that something is the case' or 'making an utterance which is capable of being true or false'.

In order to characterize the second kind of consequences, I must first digress a little. We sometimes act in a way which commits us to something; which constitutes entering into, accepting or rejecting obligations, responsibility, and liability; claiming rights or ascribing, renouncing, and transferring them. The utterances made in—not merely while—doing these things usually have the function of announcing or indicating or confirming that the person making them is committing himself, enters into an obligation, etc. But there are two essentially different ways in which this may come about, which I must explain: (i) Suppose Roberts, in a certain tone of voice, and not on the stage, etc., says one of the following: "Mr. Jones is not at home", or "You irresponsible scoundrel!" or "I'll return the book". It will then be correct for Smith to say about Roberts' statement "Roberts said (or asserted or assured me) that Mr. Jones was not at home", "Roberts reprimanded him" and "Roberts promised to return the book" respectively. And it will be incorrect for Smith to deny any of these. For utterances such as those made by Roberts are correctly *taken to be* contentions (or assertions or assurances etc.), reprimands, promises respectively, and taking them as something else, is incorrect. But this does not mean that it is so without exceptions. That it is so is no more (and no less) than a *presumption*. In the absence of any further evidence, the presumption must be accepted. But further evidence may enable us to *rebut* the presumption. Roberts may claim that he said "Jones is out" in a hesitant tone, or that he added "I think" or "probably"; he may say that after "I'll return the book" he added "if at all possible" or "probably" or "I hope", etc. All these statements, if true, would suffice to rebut the presumption that the utterances made by Roberts were correctly to be taken as contentions etc., reprimands, promises respectively. This kind of inference—the prototype of moral and legal reasoning—differs from deductive

inference in that it is based not on entailments, but on "presumptive implications", "rebuttable implications". The main characteristic of this form of implication is this. When it obtains between p and q , then if p is true, it is correct to accept q and incorrect to reject q . But other facts especially related to p and q may rebut this presumption. That is to say, these additional facts have the consequence that, even when p is true, it is no longer correct to accept q and incorrect to reject q but, as far as this presumption is concerned, neither accepting nor rejecting q is either correct or incorrect. To give an example from legal reasoning: Concerning the life of someone nothing is presumed. If it can be established that the person in question has been alive at some stage, then this is some reason for the Court to presume that he is still alive unless some evidence to the contrary is presented. But the Court is not bound by any presumptions. Only the "ordinary rules of probability" apply. However, a presumption that the person in question is dead can be established by showing that his closest relatives or any person who, in the nature of the case would be expected to have heard from him, has not in fact heard from him for seven years. It is then incorrect not to accept that he is dead. This presumption can be rebutted by showing that someone has met him recently. It would then be incorrect not to accept that he is alive. But this presumption can again be rebutted and indeed conclusively refuted by producing a witness who has actually seen him die.

This kind of reasoning is also applicable to our cases. Utterance of certain words in certain circumstances and in a certain tone of voice will correctly be taken as a contention, a rebuke, a promise respectively. But certain evidence, concerning, *e.g.* the context or what was said afterwards, etc., can rebut this presumption. It must then be presumed that the utterance was merely a surmise or the expression of an opinion, a joke or a curse, the declaration of an intention without any commitment, etc. (ii) There is another way in which it may come about that doing or saying something has consequences of the sort described. In order to set his friend's mind at rest, Roberts may himself indicate how the utterance is to be construed. This will preclude some of the ways of rebutting the presumption set up by Roberts' action. Roberts may say "*I assure you Jones is at home*" or "*I reprimand you for your scandalous behaviour*" or "*I promise to return the book*". By adding these *indicators* (the words in italics), Roberts shows unequivocally and at the cost of depriving himself of certain ways of rebutting the pre-

sumption, what sort of utterance he wants or is to be taken to have made, what exactly he is doing. By making the above utterances without adding the indicators, Roberts must be taken to be, *e.g.*, committed, but certain moves remain open to him which might enable him to show that he is not. By indicating right away what his utterance is to be taken as, he is committing himself explicitly, thereby precluding some future arguments and "defences" about and arising out of the *status* of the utterances. What rights, claims, responsibilities, liabilities, obligations are the consequence of his action, is determined by its status, which is implied (though not necessarily established) by the indicator.

Similarly, there are indicators accepting or disclaiming responsibility for a certain degree of reliance placed on the utterance accompanying the indicators. Phrases like 'I find', 'I realize', 'I believe', 'I wonder', 'I guess', 'I suspect', 'I suppose', 'I assume', 'I calculate', 'I reckon', 'I see', 'I hear', 'I feel', 'I imagine', 'I infer', 'I think', 'I discovered', 'certainly', 'probably', 'perhaps', etc. may function as indicators of this sort. Some of them, *e.g.*, 'I see', 'I hear', 'I imagine', 'I calculate' indicate more or less clearly the epistemological route or backing by which a certain statement is claimed to have been arrived at or would be supported. Others, *e.g.*, 'I guess', 'I realize', indicate merely the degrees of reliance which the maker of the statement thinks or claims may be put on it.

II

Deciding. To say that someone has made a decision is to say that he has settled an actual or possible conflict or dispute about what to do or about what is the case. *Quid* settlements, decisions are the culminations of (possible) activities of trying to settle and, therefore, of weighing of pros and cons. None are *processes* or *activities*, since they are the termini of such. But they are intended termini, and they are the termini of activities, so they are things done (brought off) and not undergone, like having recovered from measles. They ordinarily happen at moments like any other cases of reaching something. But one may sometimes come to an opinion or decision, without being able to say just when one ceased to be undecided, just as one cannot fix the moment when one was restored to complete health.

1. In one sense of 'deciding' (which I shall call sense (i)), as in "He decided to buy the house", 'deciding' is used like

'making up one's mind', 'resolving', 'choosing'. In this sense, the sense usually before the philosophers' attention, it is not a linguistic activity at all. For this reason alone it is absurd to hold, as many philosophers seem to have done, that moral utterances literally *are* decisions. For decisions in this sense are not kinds of utterances nor ways of using words, not linguistic activities at all. In making up one's mind, there is no question of using words, though one can afterwards announce one's decision. In passing a moral judgment, one must use words.

A *presupposition* of deciding in sense (i) is : being undecided. There can be no question of making a decision where there is no indecision. Indecision entails hesitation between alternative courses of action. If someone had been undecided for some time and then took a drug to end his indecision, one would not say that he had *made* a decision, had decided upon one course of action rather than another. One would not even say that he had come to a decision, but perhaps that he was cured of his indecision or that his indecision had been ended or that he had been jolted out of his indecision, etc.

Thus making a decision, deciding, presupposes getting oneself out of an indecision without the use of drugs or other *devices*. We do not speak of someone having made a decision unless he is capable of deliberating. That is to say, a person is incapable of deciding unless he was capable of deliberating in the recent past, and, if he has lost his ability to deliberate, has lost it only for a short time. Having actually deliberated is not a presupposition of deciding : we may make snap or impromptu decisions.

Another presupposition of the proper use of 'deciding' is that the sort of thing which is said to have been decided should be capable of being decided. It is equally misleading to affirm or to deny that Jones has decided to travel or decided not to travel to the moon or to Moscow, unless a space ship is leaving, etc., or Voks has invited Jones, etc. What is misleading in these cases of *impossible* decisions is the fact that the question of a decision might arise, though it has not actually arisen. This is different from asserting that someone has made a *nonsensical* decision, e.g., "He decided to stop the flow of time" or "He decided to forget his name" and different also from asserting that someone has made an *irrational* decision, e.g., "He decided to marry the first woman he met on leaving the house". Obviously it is improper to speak of carrying out a nonsensical decision but quite proper to speak of carrying out an irrational one.

A decision must be accompanied by *some* measure of success or explanation of failure, success here meaning the actual carrying out of the decision and the cessation of the deliberation or hesitation. A man may declare that he has decided to give up drinking, and he may genuinely believe that he has made the decision, but if he declares it many times and nevertheless continues drinking or deliberating when to stop, then we refuse to say that he has really decided to give up drinking, however much he may protest—and believe—that he merely succumbed to temptation each time.

Similarly, to say that someone has made a decision, involves a certain guarded prediction that he will carry out the decision, that he will not change his mind without good reason. We do not say "Jones has decided to give up drinking" if he goes on drinking, maintaining that he has frequently decided to give it up but that every time he later *changed his mind*. Nevertheless, the fact that someone has succumbed to temptation or the fact that someone has changed his mind does not entail that he has not decided. For in the cases just now discussed, we would not only refuse to say that he had decided, but also that he had really changed his mind or really succumbed to temptation. On the other hand, neither do these facts—that someone has changed his mind or succumbed to temptation—entail that he has decided to do what he later failed to do. He may merely have *intended* to do it, without having decided. But I think they do entail either that he decided or intended to do what he later failed to do. He cannot have changed his mind or succumbed to temptation if he neither decided nor intended to do the opposite.

Here is a useful general rule: when it looks as if p entails q and someone claims to produce an "exception" (r), then it is always worth considering whether it is not perhaps the case that p entails either q or r . Furthermore, it is worth considering whether the connexion between p and q is not such that, although r may be true, there is a presumption that, if p then q rather than that, if p then r . I believe many philosophical difficulties would disappear if this rule were heeded. It is true that "I see over there what looks like a chair" does not entail "There is a chair over there", for it may be that I have a hallucination, etc., etc. But "I see over there what looks like a chair" entails "There is a chair over there unless I have a hallucination, etc., etc." Moreover, there is a presumption that I do not have a hallucination, etc., etc. unless I have some particular reason to think I have.

'Deciding' is often said to be the *name* of a mental act. But

if it were, then whether someone had made a decision, would depend only on whether he had performed the mental act. We have already shown that it depends on other factors. (This says something about the nature of a 'name'.) Some people have inferred from this that 'deciding' does not refer to a mental act at all and also that there is no mental act of deciding. I want to discuss this briefly.

It is true that 'deciding' does not refer to a mental activity or process, for we cannot ask the questions "How long have you been deciding?" or "Did you decide vigorously or idly, strenuously or lazily?" etc. Deciding is accomplishing something, not trying or performing it. This shows that, if 'deciding' is ever used referringly, it cannot be used to refer to an *activity* or *process*. But it does not show that it cannot be used referringly at all. Clearly it can be so used, as in "I find deciding very difficult". It is, however, difficult to find an unmisleading word for what 'deciding' refers to. Some people probably wish to assert merely that 'act' is a misleading word to use for the referent of 'deciding' when they make the—very different—claim that 'deciding' does not refer to anything at all.

What, then, does 'deciding' refer to? Clearly something which is intimately connected with the transition from being undecided to being decided. For a decision, like a destination, may be reached at a certain moment although, unlike a destination, it need not be: "Undecided at 10 a.m." or "He decided at 10 a.m." make sense, and so does "He went to bed undecided and woke up next morning with his mind made up". Hence it is certainly right to say that 'deciding' is sometimes "episodic". From this it does not, however, follow that deciding is an act. Arriving at Paddington or understanding what someone said is an "episode", but it is not an act. Nevertheless, I think there are good reasons for saying that deciding is an act.

When we ask ourselves whether in deciding we are doing or undergoing something, whether we are active or passive, we would I think say quite naturally that we were doing something, that we were active. For of the several ways in which the transition from the state of indecision to being decided may take place, only the "active" ones would be called 'deciding'. Suppose that, when in a state of indecision about whether to marry Evelyn, Smith is given a certain sort of drug which is supposed to have the effect of ending his state of indecision. If the drug has the effect merely of steadying his nerves, reducing his anxiety, his blood pressure, palpitations, etc., this might indeed enable him to think more calmly and thus *help him* to decide this difficult

problem. If he then ends his indecision, he would still be said to have decided. But suppose now that the drug is one which is claimed to *bring about* the decision. What could such a claim mean? Not simply that the drug stops Smith from worrying about the problem, from deliberating, that it makes him forget all about it. For this sort of effect would not *be* or *bring about* either the decision to marry or the decision not to marry Evelyn. But suppose *I knew* before taking the drug which decision I would make after taking it, the decision whether to take the drug would be just as difficult as making the decision whether to marry or not. If I did not know beforehand what decision I would make as a consequence of taking the drug, it would be not unlike tossing a coin. In neither case would it be proper to say that I had made a decision, but rather that I had avoided making it. This consideration also brings out what is wrong with saying that deciding is simply the transition from a state of indecision to a state of being decided. This way of putting it conceals the logical complexity of 'deciding'. To reveal it, one would have to say 'state of being decided *about which of the possible alternatives to embark on*'.

The same point can also be brought out in this way. Suppose Smith has spent some time deliberating about something but cannot make up his mind. Suppose he goes to bed in a state of indecision and wakes up the following morning with his mind made up. We would not then say that he had made a decision about or had decided the issue albeit unconsciously. But we are quite prepared to say that he has *come to* or *reached* or *arrived at* a decision about which he cannot say exactly when he reached it, though he can say that it was not before going to bed and not after waking up.

Thus when we consider ways of passing from a state of indecision to a state of being decided which cannot be said to be "active", we also cannot properly say that these transitions are decisions. This seems to me a good reason for saying that 'deciding' refers to an act.

It may be said that if 'deciding' is said to refer to an act, then verbs such as 'see', 'hear', 'find', 'discover', etc. must also be said to refer to acts. But I think there is an important difference between seeing, etc. and deciding which makes it more misleading to call seeing than to call deciding an act. The first point is this. Both seeing and deciding are achievement words, *i.e.* words marking the success achieved in some activity, *i.e.* in looking and deliberating respectively. But achievement words like 'seeing', 'discovering', 'finding', etc. cannot

properly be used unless it is also proper to use the corresponding performance words. It does not make sense to speak of someone seeing without looking, hearing without listening, finding without picking up or looking or . . ., etc., etc. But it does make sense to speak of deciding without deliberating, etc. We are inclined to say that seeing is not an act *in addition* to looking, but we are inclined to say that deciding is an act in addition to deliberating, that deciding is not related to anything in the way in which seeing is related to looking. For this reason it makes sense to ask someone to make up his mind, but not to see or hear something, while it makes sense both to ask someone to deliberate and also to look or listen. This too, seems to me a good reason for saying that deciding, but not seeing or hearing, is an act.

What are the logical consequences of properly saying that someone had decided something? Mr. Hart¹ shows how the statement "He did it" may be used to ascribe responsibility. I think 'deciding' is used in the same way. I first state two minor modifications of Mr. Hart's view. Mr. Hart makes two claims relevant to our problem: (a) It is the primary function of "He did it" to ascribe responsibility (p. 171). (b) There are a number of defences, e.g. "Accidentally", "Inadvertently", "By mistake" etc., "which, though they do not destroy the charge ('He did it') altogether, soften it, or, as lawyers say, 'reduce' it" (p. 190).

(a) implies that "He did it" has other, secondary functions; that sometimes it may be used for these and not for the primary functions. This is misleading because *whenever* we say truly "He did it", there is a presumption that he is responsible for it and it will, therefore, always be wrong for anyone to take him not to be responsible unless this presumption is rebutted. To rebut it, he may say things like "I acted on orders" or "I acted as Smith's representative", etc. In such cases, the presumption that he is *responsible* is rebutted, but not the fact that he did it. Moreover, (a) is misleading, because it is not really the function of "He did it" to ascribe responsibility at all. That is the task of, e.g., "He is responsible" or "He is to blame". Ascribing responsibility implies knowing that one does. But "He did it" involves a presumption that he is responsible whether or not the person uttering it knows that it does. It is, however, true that someone saying "He did it" implies that he is responsible.

¹ "Ascription of Responsibility and Rights" *Proc. Arist. Soc.*, 1948.

(b) is misleading, because defences such as "accidentally", etc. do not defeat or in any way soften the charge that he did it or that he is responsible. Such defences do not alter the fact that he did it. Besides neither 'doing it' nor 'responsibility' (in this sense) are degree notions. Nevertheless these charges can, in a sense, be 'met' by these defences. For the charge "Jones did it" contains, by way of presumption, the charge "Jones is responsible" and also the charge "Jones is blameworthy to a certain degree" the degree depending on the action done. Now, some of the defences listed, e.g. "I acted on orders" rebut the presumption that, because I did it, I am to be held responsible; others, e.g. "I did it accidentally", etc. rebut the presumption that, because I did it and am, therefore, responsible (i.e. answerable = rightly called upon to answer the charge) I am either blameworthy or to be punished or, though neither of these, liable to pay.

Similarly, when we say truly "Jones decided to do it", there is a presumption that he will be responsible if he does it. Jones can rebut this presumption that he is responsible for doing it by statements such as "I acted as Smith's representative". Smith can then rebut this by statements such as "You were not authorized to make this sort of decision", etc.

2. In sense (ii), as in "You decide where we shall eat" or "The Judge decided that the Court proceedings should be secret", 'deciding' is used very much like 'arbitrating', 'giving a verdict', 'judging', etc. To do these is to say something which, in virtue of being said by the right person and in the right context and by means of the right words, has the authority to settle an actual or possible dispute. As a consequence of a decision in this sense, behaviour which is right, correct, appropriate, is authoritatively distinguished from behaviour which is the opposite. In cases where there are rules, the rules enable us in principle to distinguish these sorts of behaviour. The rules create the distinction. A decision in sense (ii) *authoritatively draws* it in actual cases. Where there is a rule, decisions thus settle actual or possible disputes arising out of divergent interpretations and applications of the rule.

3. In sense (iii), as in "When he proposed to me, I decided that he had got the job", 'deciding' is used in the same contexts but not in the same way as 'finding', 'discovering', 'seeing', 'guessing', etc. To say that someone has decided something in this sense is to assert that he has formed an opinion about something. To use the word 'deciding' in this way is to disclaim knowledge, and thereby responsibility for any consequences

that may arise out of someone relying on the opinion given. It does not follow from this that it would be either wrong, or a mistake, or foolish to place reliance on such a decision, for it may be an expert's, the best one can get in the circumstances. But it would be wrong for anyone relying on it to hold the person giving the opinion responsible for the consequences.

To illustrate. When we wish to say "Stella is two to-day", we may add some word letting it be known for what degree of reliability we are prepared to take responsibility. "I know", "I find" etc. involve me in responsibility for absolute reliability. "I had the impression", "I had the idea", "It occurred to me", etc. are fairly non-committal. If I introduce my statement by "I decided that Stella was two to-day" or "She decided he had come to ask her for money", I convey that, on the basis of the evidence at my disposal—which is inadequate—I drew the inference that Stella was two to-day, and at the same time this operates as a disclaimer of responsibility for the consequences of anyone relying on my conclusion. It should be added that this disclaimer of responsibility will not always be acceptable. One cannot always escape responsibility by simply disclaiming it: e.g. in the case of "We have decided that eating radium is harmless" said by members of a Congress of leading scientists. It is misleading to use the word 'deciding' where the evidence for the statement made is conclusive, as e.g. in "She decided that he had come to ask her for money" if he has actually said that that was his reason for coming and where she had no reason for doubting his word.

Decisions in this sense may be about whether or not something is the case, e.g. "Did he call?" or about whether or not there is a certain rule, e.g. "Is there a rule against smoking?" or whether or not someone has observed a certain rule, e.g. "Did he behave prudently?" whether a rule applies to something, e.g. "Is it permitted to smoke in here?", whether one has got the right interpretation of something, e.g. "When he wrote '32,000', did he mean '32' or '32,000'?" etc. The important thing to realize is that all these questions are questions of fact although it is most misleading to think of all of them as questions the answer to which may be established or refuted by looking, listening, etc. Yet, to all of these and many other questions, the answers may sometimes be *found*, *discovered*, etc. and not merely *decided upon* or given an opinion about. For all these cases, it makes sense to contrast deciding with finding; to say that if someone merely decided that something was the case, his opinion was less reliable than it would be if he had found out that it was the case.

These cases must be distinguished from those where it does not even make sense to ask whether someone decided or found out about something, questions the answers to which, in the nature of the case, are a matter for deciding. Such are the famous linguistic decisions, *e.g.* "Does the dog walk round the cow?" Similarly cases such as "Is this one the better proposition or that one?", where the considerations relevant to the issue are drawn from different 'planes' and there are no rules for computing the considerations, as when one car is cheap and has a good performance, but there is a long waiting-list, whereas the other is expensive, second-hand, and has a bad performance, but can be got right away. In these cases there is no question of finding out, hence there can be no question of contrasting deciding with finding out. We cannot say "He merely decided, he did not find out". Nor can we say "He decided arbitrarily", *i.e.* did not weigh the considerations. For he did weigh the considerations, but not on the same balance, and they felt as if they had the same weight. I think it is this unfortunate high-lighting of cases which are not a matter for finding out at all and cases where all the considerations seem to have the same weight, which has recently given the notion of decision its peculiar and startling flavour, making it the darling of the Existentialists.

4. Decisions, particularly in sense (ii), are often confused with, and must be distinguished from, rules, laws, and commands. Rules and laws are primarily devices for bringing about settlements of public conflicts and disagreements, decisions are the authoritative settlements themselves.

4.1 Rules may be discovered, stated, formulated, communicated or laid down, interpreted, amended, recommended, enforced, etc.; they may be adopted, heeded or disregarded, observed, infringed or violated. The differences between the various kinds of rules are best understood by reference to *regularities*. We must distinguish between regularities of natural events and regularities of human behaviour. Regularities of natural events can be discovered, stated, formulated, and communicated. The formulations of these regularities are one kind of rules, *e.g.* "(As a rule), explosions are accompanied by noise". Regularities of this sort call for causal explanations, *i.e.* for more general rules from which those can be derived and which enable us to say that, given a number of events, another event *must* happen. The regularities of human behaviour are different. Where men are involved in the previous sort of causal regularity, as when a bus full of people falls off a bridge, we do not

speak of human 'behaviour', but of 'events in which humans are involved'. About regular human behaviour we can always ask the question why it is regular. It always makes sense to say "It is his habit" or "It is his routine", etc. It is nonsense to speak of the habits, etc., of the stars, hence nonsense to ask for this sort of explanation of the regularity of their motion (not behaviour). Failure to see the implications of this is responsible for the misleading term 'scientific law' and for some of the problems associated with induction. On the other hand, regularities of human behaviour may be *due to rules*. If someone acquires a certain habit or routine without thinking about it or without perhaps even knowing it, this regularity is not due to a rule, nor would we say that he has a certain rule nor that so-and-so is his rule, though it would be right to say that he did so-and-so *as a rule*. If, on the other hand he knowingly adopts a certain practice or habit or routine, such as winding up his watch before going to bed, then his behaviour is due to an adopted rule. The keeping of such an adopted rule may become quite automatic after it has been adopted. Such a rule does not exist except in virtue of being adopted by someone and it exists only for him. It is his rule because he has adopted it and while no one else adopts it only he can keep it.

But adopting and keeping such a rule may itself be prescribed by a rule of quite another sort. Examples of this type of rule are: rules of prudence (telling us *what* to do *when* and *where*) or rules of skill (telling us *how best* to do it), or rules of etiquette (telling us what it is *proper* to do), or rules of morality (telling us what it is *right* to do). Rules like these cannot be adopted, but they can be heeded or disregarded, observed, infringed or violated, and heeding them may consist in adopting a certain routine, etc. That a certain rule is a rule of skill or prudence does not impose on us an obligation to observe it, though it is a good reason for us to heed it. Other rules, *e.g.* the rules of morality and the laws of our country, do impose an obligation on us to observe them, whether we have other good reasons to heed them or not. Among these latter rules which cannot be adopted and which can not, therefore, exist simply in virtue of being adopted, we must distinguish those which come into existence by being laid down, like the laws of our country, and those which do not, *e.g.* the rules of good taste.

The rules which state regularities of natural events can be known but not observed. They may be heeded, but this merely means that they may be useful in warning us of some impending event, as when someone heeds the rules of the tides by not

swimming at certain times. Others, like the rules of skill and prudence, may be heeded, *i.e.* known and followed, or disregarded, *i.e.* known and not followed. Yet others, like the rules of morality, etiquette, etc. may be observed, *i.e.* known and followed. "Jones observed the rules of Samoan etiquette without knowing them" means what is more properly expressed by "Jones behaved as if he knew and meant to observe the rules of Samoan etiquette". Or they may be infringed, *i.e.* not followed whether known or not known. Or they may be violated, *i.e.* known and not followed. "Jones did not observe the rule" leaves open the question whether Jones infringed the rule without knowing it or whether he violated it.

4.2 "What is the difference between a rule and a command?" is a somewhat misleading question. For it suggests that commands and rules differ in the sort of way in which footballs and tables differ. But the statement "This is a rule" may be used in many different ways. It may be used like "This is a sentence of a certain grammatical form" or "This is a sentence capable of being used in stating, or formulating, or communicating, or laying down a rule"—and it may not be realized that sentences stating a rule may differ in form from sentences laying down a rule—or "This is a routine, habit, maxim, etc. adopted by someone" or "This rule exists".

"This is a command", on the other hand, normally implies "This is a linguistic activity of a certain kind, *i.e.* giving commands". This says *e.g.* something about the relation of the person commanding to the person addressed. It contrasts this relation with that of a person who, in uttering the same sentence, is requesting, or suggesting, or appealing for something, or giving advice or making a demand or a humble supplication. In those cases, the required response is not called obeying, but complying, accepting, responding, following, acceding to, granting, respectively.

Now, an important difference between 'rule' and 'command' is this, that while 'command' is normally used to refer to or to characterize linguistic activities of a certain kind, *i.e.* commanding, the noun, 'rule' (unlike 'ruling') is never so used. Hence while 'command' on the one hand and 'request', 'appeal', etc. on the other, differ in important respects, they yet have this in common, that they may be used to refer to or to characterize linguistic activities of a certain sort. There are, however, two ways in which the similarities and differences between 'rule' and 'command' may be further clarified. In the first place, there is a linguistic activity which, at first sight, looks very

much like commanding, *i.e.* laying down rules. Secondly, both 'command' and 'rule' have been used as names for certain kinds of sentence.

I think that giving commands and laying down rules are activities which are different in very important respects. Commands are addressed to people, rules that have been laid down apply to them. Such rules must be declared invalid, commands are cancelled or countermanded. Two people on two different occasions may give the same command to the same person(s), yet two commands will have been given. If they are obeyed on both occasions, two commands will have been obeyed. Two people cannot, however, lay down the same rule for the same person(s). If they tried, they could succeed at best in laying down only one rule. If two people observe such a rule, they observe one and the same rule. Rules may be observed, infringed or violated. Commands may be obeyed or disobeyed or they may not be obeyed. To say "He disobeyed the command" implies "He knew that a command had been given, had heard and understood what it was, *i.e.* had heard the words and knew what would constitute executing the command, knew how to go about executing it, was able and free to execute it, and did not execute it". To say "He did not obey the command" merely implies that a command was given and that he did not execute it. We blame a man for disobedience, never merely for not-doing-what-has-been-commanded. In a case of non-obedience, anything showing that any of the features of disobedience are absent, may be offered as an excuse for the non-obedience. The difference between disobedience and violation of a rule is this: disobedience is rebellion against the authority of a person, violation of a rule is engaging in the wrong sort of activity. Occasions for obedience arise at the pleasure of the commander, occasions for observance of a rule arise out of the events themselves.

Both 'rule' and 'command' have been used, especially by philosophers, to refer to the sentence or the type of sentence involved in commanding and formulating a rule respectively. This shift of usage is made more natural by the fact that certain commands, *e.g.* military commands, must be given by means of a special formula. Thus, we distinguish between military commands, like "Attention!" and an order like "Go and clean your boots, Pte. Jones!" But being a sentence of the right sort for use in commanding or formulating a rule is merely a necessary, not a sufficient condition for calling something a command or rule. But it is true that some sentences are incapable of being

used for giving commands or formulating rules. There are two ways in which sentences demanding or prescribing behaviour may differ in generality: (a) The way the person(s) *meant* are indicated. They may be indicated directly, by being spoken to, or by the mention of the name(s) of the person(s) meant, as in "Meet me at six!" or "Tell Jones to bring me the file on H & S"; or indirectly, as in "All men above 18 will register". (b) The way time and place of the action prescribed are indicated. It may be indicated directly, *i.e.* by specifying the occasion, as in "Meet me outside the King's Arms at six!"; or indirectly, *i.e.* by not specifying the occasion, as in "Keep off the grass!". Now, no sentence can be used for laying down rules *if* it is particular either in respect (a) or in respect (b), and none can be a command *unless* it is. "As from to-day, summer uniforms will be worn" may be used to lay down a rule, because it may be general in respects (a) and (b). But it may also be a command, because it may be particular in respect (a), if it is read to all ranks in an Order of the Day. To lay down rules is very different from formulating them. A sentence may be the formulation of a rule, though it is incapable of being used in laying down a rule, if it is particular in respect (a) and general in respect (b). 'Doctors' orders' will illustrate this: "Jones, you must always do this and never do that unless you have first done the other!" The doctor is indeed *formulating* a certain rule and he *orders* the patient to make it his rule, to adopt the routine or practice or habit stated in the rule. The patient does not *spontaneously* adopt the routine, etc. nor because *there is* a rule which he has good reason to heed or even an obligation to observe but because the doctor orders him to do so. This is not easily seen because, in the ideal case, the routines, etc. stated in the doctor's orders are instances of yet other sorts of rules, rules stating what anyone in a certain condition must do if he is to retain or regain his health. If the doctor does his job properly, then his order to Jones will state a routine which anyone in Jones' position must adopt if he is to retain or regain his health. Hence doctors' orders, in the ideal case, are *also* statements to the effect that certain general rules apply to Jones. They are also the formulations of these general rules. However, doctors' orders are given in the form of orders. Therefore, whatever else they sometimes are and always ought to be, they are orders which may be obeyed or disobeyed and they are not the laying down of rules.

4.3 When would we say that a certain rule existed or that there was a rule to the effect that . . . ? There is a plausible answer: a rule exists when it has been properly laid down, *i.e.* laid down

by the right person, on the right occasion, with the right ceremony, in the right words. Call such a rule an "imposed" rule. A rule, if thus laid down, is valid, otherwise invalid. But this answer has misled those who do not realize that there are rules which cannot be laid down and which are, nevertheless, quite different from merely imaginary rules or invalid rules. These people, mistakenly, look for the ground of the validity of all rules. But rules which there is no question of laying down, either properly or improperly, cannot be either valid or invalid.

Thus, the validity of a rule implies its existence, though not *vice versa*. The same holds good for the *effectiveness* of a rule. Roughly, to say of a rule that it is effective is to say that it is observed by a higher proportion of people to whom it applies than is a certain standard rule. There may be different standards of effectiveness, just as warm soup and warm beer have different standards of hotness. But just as, by the use of a thermometer, we can say that cold soup is "warmer" than warm beer, so by a common standard of effectiveness, *i.e.* proportion of observance by people to whom it applies, we can say of one effective rule whether it is more "effective" than another effective or ineffective rule.

'Being effective' must not be confused with 'being binding'. That a rule is binding implies that it exists but it does not imply that it is effective. And a rule may be effective without being binding. To say of a rule that it is binding is not only to imply that it exists but also that it is a good thing that it exists.

We would not say of a "non-imposed" rule that it existed unless deviations from it were widely considered to be violations of the rule. As we have seen, the fact that people have habits, routine, etc. that they have common practices or even maxims, does not entail that there are rules. For there is a big difference between saying on the one hand, "As a rule, capitalists put profit above everything" or even "It is a maxim of all capitalists to put profit above everything" and on the other, "There is a rule that capitalists should put profit above everything". The difference is simply that while each capitalist may in fact act in this way or may have adopted this maxim and may know that every other capitalist acts in this way or has adopted this maxim, this maxim is yet not a rule unless deviations from it are considered violations. Capitalists may heed or disregard these maxims, they could not be said to observe or violate them.

Again, one would not say that a "non-imposed" rule existed unless it were widely understood and formulated. As we have seen, habits, routines, maxims, principles, are rules of individuals,

of particular people. We could not speak of the habits, etc. of societies or countries. The proper term for that is custom. For customs are not acquired or adopted by individuals separately. The individual *finds* them already in existence. But a country or society may have customs, and the members may *conform* without being clearly aware of what the customs are. A social scientist arriving in the area of a primitive tribe might know nothing of their way of life. He would at first attempt to *imitate* their behaviour. Later, he would learn to predict what they would do and what was expected of him. He would learn to conform. Being able to conform, like being able to speak a language or playing a game, does not presuppose being able to formulate the rules. There might be tribes in which almost every member conformed, but no one was able to formulate the rules. I do not think we can speak of there being rules unless they are formulated. And it is not enough that a visiting scientist should have formulated them. The people of the community must know them. For when a rule exists it must be possible to observe it. But observing it implies knowing *what it is*. (I may know and conform to the rules of chess—but I cannot observe them—without knowing *what* they are.) In the absence of such knowledge we can speak only of conformity.

Moral principles and moral laws differ from moral rules in that the former need not be recognized. We speak of there being a moral law against slavery, or of the principle embodied in the *Golden Rule*, whether anyone recognizes it or not. We can say of a moral rule, but not of a moral principle or law "It came into existence during the Renaissance". A moral law or principle becomes a moral rule by becoming recognized. Thus, although in ancient Greece, there was no rule of any sort against slavery (and, therefore, Greeks in keeping slaves, did not violate any such rule), we might yet say that *there is* (in the tenseless sense of the verb) a moral law against slavery. Hence it *was wrong* for the Greeks to keep slaves, although it was also perhaps excusable, because it did not occur to them that there was such a law. (The term 'moral law' is somewhat misleading because 'law' seems to imply 'legislator', but moral laws are not created or destroyed by being "laid down" or "abrogated", by God or anybody.)

4.4 This may be used in clearing up a misunderstanding concerning the phrases 'ordinary usage' and 'ordinary use' which, if mentioned at public meetings, frequently cause the fruitless fraying of tempers. Certain philosophers urge us to examine ordinary usage, with a view to raising it to a norm.

But this method is widely misunderstood, I think. What people do in fact say, is part of what is being examined and it is undoubtedly what we must go by when deciding what it is correct to say *unless it constitutes a violation of an ordinary use*. To illustrate. People frequently use the phrase "From the beginning of time . . .". Now, as ordinarily used, this is a harmless idiom, meaning something like "For as long as we can remember . . ." or " . . . always . . .". But philosophers and laymen alike have subjected this idiom to grammatical analysis as if it were a phrase like "It looks like a meteor" and not like "It looks like war". But so taken, it is easily seen to constitute a violation of the ordinary use of the phrase 'From the beginning of . . .' and the word 'time'. To violate a use is either to violate a linguistic rule or to speak in a way which would constitute violation of a linguistic rule if the rule contained in certain linguistic habits, etc. were made explicit. Now the following rule is perhaps not generally recognized, but it seems to me there can be no doubt that it makes explicit our linguistic 'customs' concerning the words and phrases in question. I mean the rule that "From the beginning of so-and-so" entails "Something came before so-and-so", and the rule that "Time does not exist" entails "The words 'before' and 'after' have no application". Hence, according to one rule, it makes sense to ask "What happened before the beginning of time?", according to another it does not. As I understand it, the purpose of investigating ordinary usage is not only to find out what people actually do say, but to examine whether what they do say constitutes an unrecognized infringement of linguistic rules which either are generally recognized but whose application to the case in question is not, or which are not generally recognized but which are, nevertheless, the correct formulations of our linguistic customs. This is not an ordinary empirical inquiry nor is it the laying down of rules. It is making our linguistic customs explicit.

5. We can now see what is the difference between a decision in sense (ii) and a rule on the one hand, a command on the other. The existence of a rule *enables us* to settle an actual or possible conflict. A decision actually does it. Like laying down a rule, but unlike giving an order, deciding presupposes the right to do so. On the other hand, a decision is like a command in that it is particular in both respects (a) and (b) mentioned above. True, a Court may be said to have decided on a general principle. But this only confirms my point. For as jurists have pointed out, in that case Judges arrogate to themselves—perhaps rightly, perhaps wrongly—the right to *lay down general rules*,

often of the sort reserved to the legislature. And in doing so they are not so much deciding the present case as enabling other judges to decide later cases.

6. Let us now examine whether moral utterances are decisions. Of course, not all moral utterances are of the same logical type. We may make moral judgments, as in "He ought not to have done it", etc. or we may state moral principles, as in "Do as thou would be done by", etc. or we may state moral laws, as in "Murder is wrong", etc. or we may give reasons in support of moral judgments, as in "He ought not to have done it. It hurt the girl", etc. There are other types, but the ones mentioned seem to be those mostly thought of by the philosophers. Now, since all these are linguistic activities and since deciding in sense (i) is not a linguistic activity, none of these can be classed as decisions in sense (i). Still, making any of these moral utterances may—but it need not, for one need never have been undecided—*involve* making a decision in this sense, *i.e.* the decision to make the utterance. But making the moral utterance cannot be the decision to make the moral utterance, nor can the moral utterance itself be *that* decision. Yet it seems to me that most of those who have held that moral judgments or statements or utterances or principles are or involve decisions, have meant just this.

Nor are moral utterances decisions in sense (ii). For, unlike judicial decisions or friendly arbitrations or paternal decisions etc., moral judgments have no settling authority. Unlike a decision and like an assertion or constitution, a moral judgment settles nothing. It is either correct or incorrect, but anyone can arrive at a correct moral judgment, and no one's moral judgment is any the more decisive or incorrigible for being his. It is true that a moral judgment in a sense "announces where I stand". But so does an assertion. Where there is disagreement, *e.g.* whether the armies of the Mauves or those of the Greens first crossed the frontier between them, to assert that the Mauves did, or that the Greens did, not merely states what happened, it also "announces" (in this sense) what the asserter believes. Neither is there, as far as I can make out, another sense of deciding in addition to sense (ii) which is in all respects like sense (ii) except that it lacks the characteristic of being the authoritative settlement of some actual or possible dispute. Nor are the statements of moral principles or laws or the giving of reasons, decisions in this sense, not even "social" decisions. Where a moral principle or moral law is made part of a legal system through the decision of a legislator (a "social decision"), this involves a decision in sense (ii). But

the principle or law incorporated by this decision is not thereby created. Yet it seems to me that Stevenson must have meant that moral utterances *are* or in a most intimate sense *involve* decisions in sense (ii) when he said (*Ethics and Language*, Preface) "... normative ethics is more than a science. . . . Such a view does not require a faith in some higher type of knowledge beyond that to which the sciences can attain. It requires only the realization that ethical issues involve personal and social decisions about *what is to be approved*, and that these decisions though they vitally depend upon knowledge, do not themselves constitute knowledge."

No one, as far as I can make out, has said anything to suggest the view that moral utterances are decisions in sense (iii). Yet what plausibility the view has that moral utterances are decisions rests on the vaguely understood meaning of 'decision' in sense (iii), that of forming a mere opinion. For what strikes people most when considering moral utterances, is their own inability to "prove", or even to say how one *might* support them. And since deciding in this sense is usually contrasted with "finding" or any of the other trusted ways of coming to know, it is plausible to regard moral utterances as decisions, *i.e.* as mere opinions. But this is a mistake. There are some moral judgments, principles, and laws which we have every reason to accept, whereas there are others which are doubtful, just as there are some statements of empirical fact which we have every reason to accept and some which are doubtful. It is simply absurd to hold that there are not some moral judgments etc. about which we are incomparably more certain than about others. *E.g.*, that someone does something wrong who, walking down High Street, suddenly whips out a pistol and shoots passers-by at random, is much more certainly correct than the judgment that someone does something wrong who, in order to alleviate someone's suffering, gives him morphia with which to kill himself. Yet, if this is so, it is most misleading to hold that moral utterances *are* decisions in this sense, as if being a mere opinion was a logical feature of moral utterances.

There may be another sense of 'decision' which I have overlooked and which these philosophers have in mind. It will perhaps be admitted that, if there is such a sense, it is not a very obvious one and requires to be more clearly explained and carefully distinguished from the above uses than has been done so far.

I want to conclude this section with a few remarks about judicial decisions. Judicial decisions are decisions in sense (ii). It has been argued that because of this, they cannot be descriptions and

cannot, therefore, be true or false but merely right or wrong (cf. Hart, *op. cit.* pp. 181-82). I want to argue that some judicial decisions may be true or false. Sometimes a jury or a judge has to "find" on a question of fact, say that the accused has put poison in his wife's coffee. Now, as an utterance authoritative in the way in which a decision in sense (ii) is, it may be correct or incorrect, *i.e.* arrived at or not in the way the law requires. But as a contention (however authoritative) about a matter of fact, it may be true or false. And such decisions (or "findings") really are about matters of fact, for we would speak of a judicial error and, in a flagrant case, of "judicial murder", if it later turned out that, say, it was the servant who put the poison in the coffee. But it is not only decisions concerning matters of fact that may be true or false. When the judge decides that Smith is guilty of murder, then apart from this decision being correct or incorrect in the legal sense, we may also ask whether he really is guilty, for these words *have an ordinary meaning*. It is misleading to say "What cannot be said of it (the decision) is that it is either true or false". (Hart, *op. cit.*, p. 182.) For while truth and falsity are not attributes which the judge's utterances may have *qua* decisions, this is due merely to the fact that the judge's utterance is a decision in sense (ii), *i.e.* is authoritative, final. This has the consequence that we may say of some judicial decisions that they are false. But we cannot, on that ground alone, say that they are *wrong*. Outside a Court of Law we should have to say that of any decision. To put it crudely, it may be impossible to get a decision altered simply because the statement constituting the decision is false. There must be a ground of appeal if the decision is to be reconsidered, and in the absence of (legally) adequate fresh evidence, the decision will stand, even if it were in fact false.

It is, however, true that there are judicial decisions which are neither true nor false, *e.g.* "He shall hang".

III

Describing. I have tried to show in some detail how the ordinary use of the words 'deciding' and 'decision' differs from the way these words have been used by some philosophers, and how this deviation from ordinary use has led to important errors. I have not the space to do this in equal detail for 'describing', 'description', and 'descriptive'. I can merely indicate by giving examples, in the way I imagine Moore might, how very paradoxical is the view that moral terms are not

descriptive, moral utterances not descriptions. The full story must be left to be told elsewhere.

Is it not patently absurd to say that terms like 'fair', 'courageous', 'just', 'noble', 'charitable', 'mean', 'spiteful', etc. are either not moral or not descriptive; that sentences containing these words cannot, for this reason alone, be descriptions? Is it not, on the contrary, a plain fact that, without such words, we could not describe people's characters at all or only very clumsily? Is it not plain that it must be a perverse theory which does not allow us to say, that *e.g.* "He has a saintly nature" or "He is an habitual thief" accurately describes the man? Is it not obvious that a theory must be false which turns the perfectly clear phrase "a moving (or evocative) description" into a contradiction in terms?

And is it not evidently untrue that, in making any of the various kinds of moral utterance, we cannot ever be describing something? Admittedly, passing a moral judgment on something is not the same thing as describing it. Clearly, these are two different ways of using language. In doing the one, we are not, *eo ipso*, doing the other. But it does not follow from this that we cannot use the same words or sentences for either purpose. Nor does it follow that we cannot use moral terms or sentences containing them for both purposes at the same time. I may both describe Jones and pass moral judgment on him when I tell my friends that he is a saintly type or that he is a lecherous old man.

Thus it seems to me quite plain, even on the most superficial investigation, that it is incorrect to hold that moral terms are not and cannot be descriptive, and that moral utterances are not and cannot be descriptions, unless by this one wishes to maintain merely the triviality that 'moral utterance' does not mean the same thing as 'description'.

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IV.—ETHICAL DISAGREEMENTS AND THE EMOTIVE THEORY OF VALUES

BY VINCENT TOMAS

SUMMARY accounts of variants of the so-called "emotive theory" of value statements have been published by C. D. Broad,¹ Rudolph Carnap,² Bertrand Russell,³ A. J. Ayer,⁴ and others, but Charles L. Stevenson's *Ethics and Language*⁵ is, so far as I am aware, the only extensive work attempting to present an emotive theory systematically and in detail. In what follows, I shall for this reason be concerned with Stevenson's version of the theory. It differs in many respects from the others mentioned; yet, as Stevenson says, "it finds much more to defend in the analysis of Carnap, Ayer, and the others, than it finds to attack. It seeks only to qualify their views—partly in the light of Dewey's—and to free them from any seeming cynicism" (p. 267). Its arguments and general conclusions may therefore be regarded as typical of an emotive theory, and a discussion of them should be of import not only for an evaluation of Stevenson's own view, but of emotive theories in general.

Before looking into the validity of these arguments and conclusions, it should be observed that the emotive theory has been discussed primarily as a contribution to moral philosophy. This may be attributed at least in part to the fact that proponents of the theory tend to *illustrate* it mainly by means of statements expressing judgments of the kind we ordinarily classify as moral, but the conclusions based upon the analyses of these statements are much broader in their scope. The theory ostensibly accounts for not merely moral judgments, but normative judgments of any sort. In *Philosophy and Logical Syntax* Carnap exiles all value statements to the realm of metaphysics (p. 26). What appears in the first edition of Ayer's *Language, Truth, and Logic* as a "critique of ethics" is explicitly designated, in the introduction

¹ "Is 'Goodness' a Name of a Simple, Non-natural Quality?" *Proceedings of the Aristotelian Society*, Vol. xxxiv, 1934.

² *Philosophy and Logical Syntax*, 1935, pp. 22-26.

³ *Religion and Science*, 1935, chap. ix.

⁴ *Language, Truth, and Logic*, 1936, chap. vi, and the introduction to the second edition, 1946.

⁵ Yale University Press, New Haven, Conn. (Oxford University Press, London), 1945.

to the second edition, "The Emotive Theory of Values". It may be contended that when Ayer says "theory of values", he means "theory of moral judgments"; that when he says questions of value are all questions of taste, he means that disagreements about whether an action is morally right are at bottom disagreements in taste; and that when he says "the expression of a value judgment is not a proposition" (p. 22), he means "the expression of a moral judgment is not a proposition", and has only failed to make the distinctions clear in his own mind. Stevenson, at any rate, is as explicit as could be desired on the point that when he says "ethical judgment", he generally does *not* mean "moral judgment", but "value judgment", of any sort. He says, "The moral senses of ethical terms are no more interesting, for our purposes, than the nonmoral ones; for the topics they introduce raise no special problems of language or methodology" (p. 92). His analysis of "good" is intended to fit not merely the sense of the term which "abbreviates 'morally good'", but "common garden variety" senses of the term, "similar to that of 'swell' or 'nice'" (p. 90). In a footnote he explains that "'ethical analysis', as here understood, includes most of what R. B. Perry would call the 'theory of value'" (p. 92). Even this does not describe the breadth of Stevenson's project accurately. As judged by his insistence upon the ineradicable vagueness and ambiguity of "ethical" terms (*cf.* p. 34), he wishes to present patterns for analysis of the meanings of "good", "right", "ought", and so on as used by anyone in any context whatsoever. Since it seems impossible to avoid speaking of good and bad arguments, of right and wrong methods of inquiry, and of rules of logic or of prudence that we ought to follow, not only should the foundations of morality "tremble at a whiff of epistemological grape-shot" from the emotive theory, as Winston H. F. Barnes has said,¹ but so should the foundations of a good deal else as well.

1. *Summary of Stevenson's view.* Stevenson sharply contrasts ethical disagreements and disagreements in belief. Cases of the latter kind, he says, "require only brief attention" (p. 2), and their nature is meagrely described. They are the "disagreements that occur in science, history, biography and their counterparts in everyday life. . . . Questions about the nature of light transmission, the voyages of Leif Ericsson, and the date on which Jones was last in to tea, are all similar in that they may involve an opposition that is primarily of beliefs. . . . In such cases

¹ "Ethics Without Propositions", *Logical Positivism and Ethics*, Aristotelian Society Supplementary Volume xxii, 1948, p. 1.

one man believes that p is the answer, and another that not- p , or some proposition incompatible with p , is the answer; and in the course of discussion each tries to give some manner of proof for his view, or revise it in the light of further information. Let us call this 'disagreement in belief' (p. 2).

A disagreement in belief differs from an ethical disagreement in that "the former is concerned with how matters are truthfully to be described and explained; the latter is concerned with how they are to be favoured or disfavoured, and hence with how they are to be shaped by human efforts" (p. 4). When two persons disagree in their judgments of value (have an "ethical" disagreement), they disagree in their attitudes toward the object they are evaluating, "one approving of it, for instance, and the other disapproving of it" (p. 3). "It is disagreement in attitude . . . that chiefly distinguishes ethical issues from those of pure science" (p. 13). We may distinguish between two kinds of ethical disagreement. (1) The first kind consists of those cases in which the disagreement in attitude is entirely the result of disagreement in belief. "Suppose that A and B have convergent attitudes towards the *kind* of thing that X *actually* is, but indicate divergent attitudes to X simply because A has erroneous beliefs about it, whereas B has not. Discussion or inquiry, correcting A's errors, may resolve the disagreement in belief; and this in turn may be sufficient to resolve the disagreement in attitude. X was an occasion for the latter sort of disagreement *only* because it was an occasion for the former" (pp. 5-6). (2) The second kind of ethical disagreement consists of those cases in which the initial disagreement in attitude is ultimately the result of basically divergent attitudes, and which persists even when both parties agree in belief about the nature and consequences of X. For "there may be disagreement in attitude without disagreement in belief. . . . A and B may both believe that X has Q, for instance, and have divergent attitudes to X *on that very account*, A approving of objects that have Q and B disapproving of them" (p. 6). Whether it is of type (1) or type (2), an ethical disagreement, as contrasted with a disagreement in belief, is one that involves an opposition in attitudes.

The role of attitudes in ethical disagreements is reflected in the meaning of ethical statements, which differ from scientific statements in that they "have a meaning that is approximately, and in part, imperative" (p. 26). They are "concerned with recommending something for approval or disapproval" (p. 13), and they "are used more for encouraging, altering, or redirecting people's aims and conduct than for simply describing them"

(p. 21). Accordingly, "Any definition which seeks to identify the meaning of ethical terms with that of scientific ones, and does so without further explanation or qualification, is extremely likely to be misleading" (p. 20).

To preserve the quasi-imperative, or emotive, meaning which is an essential feature of ethical terms, Stevenson submits two patterns of analysis for determining what value terms mean as used by various people in various contexts. On the first pattern of analysis, a statement of the form "X is good" strictly designates "I approve of X" and suggests "Do so as well". On the second pattern of analysis, it strictly designates "X has qualities or relations P, Q, R" and suggests "I approve of X; do so as well".

The fact that ethical statements have emotive meaning is advanced by Stevenson to explain an important difference between disagreements in belief and ethical disagreements, namely, that whereas the question which (if either) of two conflicting beliefs is correct can be settled by rational methods, whatever reasons are advanced to support or attack value judgments are "related to them psychologically, rather than logically" (p. 115). The reasons one gives to justify a value judgment "represent efforts to change attitudes, or to strengthen them, by means of altering beliefs. Hence, although the reasons themselves are of an empirical character, and may be rendered probable or improbable by scientific methods, one must not say that they render the ethical judgments 'probable' or 'improbable' in the same sense. They are simply of a sort that may lead one person or another to have altered attitudes in consequence of altered beliefs, and so, thereafter, to make different ethical judgments" (p. 118).

The conceptions of the nature of disagreements in belief, of ethical disagreements, and of how they differ from one another, are of crucial importance for the emotive theory. Stevenson has written, "My methodological conclusions centre less on my conception of meaning than on my conceptions of agreement and disagreement. If the solution of normative issues requires agreement in attitude, if the relation between attitudes and beliefs is causal and possibly subject to individual differences, and if rational methods can affect agreement in attitude only through the indirect method of altering beliefs, then the essential features of my analysis remain intact."¹ I wish, therefore, to comment first of all on the nature of disagreements in belief.

¹ "Meaning: Descriptive and Emotive", *The Philosophical Review*, Vol. lvii, No. 2, March, 1948, p. 142.

2. *Disagreements in belief, no less than ethical disagreements, involve questions of appraisal.* If disagreements in belief "require only brief attention", as Stevenson says, it is only because he takes it for granted that when two men differ in opinion about a matter of fact, no question of appraisal is involved. But, I submit, if a difference of opinion is to be an occasion for dispute, it must also be a difference of *appraisals* of opinion. Unless when A asserts *p* he also suggests that to believe *p* is "correct", whereas when B denies *p* he also suggests that to believe *p* is "incorrect"—unless, in short, A and B disapprove of each other's opinions—they have no reason to argue. As Peirce showed in "The Fixation of Belief", and as Stevenson implies when he says that a disagreement in belief "is concerned with how matters are *truthfully* to be described and explained" (my italics), the real issue between the disputants is, "Which opinion toward *p* is correct?" And the discussion or inquiry in which they engage when they try to "prove or disprove *p*" is a process by means of which they endeavour to decide whether belief or disbelief in *p* is correct.

Suppose A says, "Jones was last in to tea on Sunday", and B replies, "No, he was not". B's utterance not only formulates his disbelief of *Jones being last in to tea on Sunday* but also suggests, among other things, that belief that Jones was last in to tea on Sunday is incorrect. What A and B are trying to do, when "in the course of discussion each tries to give some manner of proof for his view, or revise it in the light of further information", is to justify or rectify their opinions. On a question such as, "Was Jones last in to tea on Sunday?" they would probably have little difficulty in reaching a mutually acceptable decision as to the "true" answer, and therefore as to whether belief ("Yes") or disbelief ("No") was the correct epistemic attitude to have towards the proposition. For both would tacitly follow the same rules of procedure for finding out the answer. Each would search his memory, consult his appointment calendar, get testimony from the maid or from Jones himself, and then, if necessary, would revise his opinion according to what both agree is to be called "the evidence". They can do this because for both of them the correct epistemic attitude to take toward *Jones being last in to tea on Sunday* is that of belief, if the evidence from memory, observation, and testimony renders it probable, and disbelief, if that evidence renders it improbable. In short, both of them regard the same sort of reasons as good reasons for believing or disbelieving that Jones was last in to tea on Sunday.

3. *Disagreements in belief, no less than ethical disagreements, are of two kinds.* This brings us to something else that Stevenson takes for granted, *viz.*, that whenever two people differ in opinion about a matter of fact, they do not disagree about the criteria by appeal to which a rational decision can be reached as to which opinion (if either) is "correct". But disagreements in belief, like what Stevenson calls ethical disagreements, are of two kinds: (1a) The first kind consists of those cases in which the parties who disagree both regard the same sort of reasons as good reasons for believing or disbelieving something. A may believe *q* because he believes *p*, and that *p* implies *q*, while B may disbelieve *q* because he disbelieves, or is ignorant of, *p*, or the fact that *p* implies *q*. Such disagreements are susceptible of being settled by A and B coming to agree about *p* and its logical relations to *q*. They are "merely" disagreements in belief. Thus, A may believe and B may doubt that Leif Ericsson made a voyage to North America because A believes that the Stone Tower at Newport was constructed by Norsemen, whereas B disbelieves that it was. Both are agreed that if it were established that the tower was constructed by Norsemen, this would be a good reason for believing that Ericsson made a voyage to North America. (2a) But not all disagreements in belief occur between people who are tacitly agreed as to how the disagreement should be settled. Suppose, for instance, that the question about the correct answer to which A and B disagree is, "What is the nature of light transmission?" And suppose, further, that A, who believes *p* is the answer, has what he calls the scientific attitude towards this question. What will be the nature of their disagreement if B should contend that A's belief is incorrect, and give as his reason that *p* is contradicted by something said in the Bible?

The preceding example shows that a disagreement in belief is susceptible of being settled by scientific method, as Stevenson assumes all such disagreements are, if, and only if, the parties who disagree are "scientists": (i) They must acknowledge (at least implicitly) the same set of rules as defining "scientific" method for deciding whether a proposition is probable or improbable, *i.e.* *ought* or *ought not* to be believed by rational minds; and (ii) they must abide by these rules, in the sense that their beliefs are actually determined by "scientific" reasons, and not merely by causes, such as knowledge that a hypothesis is contradicted by a Biblical text, which are not evidential by the criteria of science. Clearly, if condition (i) is not satisfied, as for instance it very often was not satisfied during the great controversy

over the Darwinian theory, the "proof" or "information" advanced in the course of discussion by either of the disputants may not be accepted by the other as a good reason for regarding his own opinion as incorrect.

Disagreements in belief between disputants who are not agreed about the norms by which the correctness of beliefs is to be judged are rather more common than seems generally acknowledged by writers on the methodology of science. For them, scientific method is *the* method for settling disagreements in belief, and their problem is to explicate the rules which, they presuppose, everybody follows or, if he does not, ought to follow. The intuitionist, the authoritarian, and, as judged by current accounts of Soviet views on genetics, the "Marxist scientists", who violate these rules, are declared to be "unscientific", or "irrational", or "misusing reason". And so they are, as judged by the norms that the one who applies these epithets himself acknowledges as governing "correct" thinking. However, it is not necessary to invoke examples as extreme as those mentioned. It is notorious that disagreements in belief which are rooted in normative disagreements abound in philosophy and in the social sciences. In psychology, the issue between extreme behaviourists and introspectionists is not a "mere" question of fact, but a dispute as to what sort of thing ought to be regarded as fact. And in Tennessee, the state legislature has still not seen fit to acknowledge as "really" correct the scientifically correct theory of evolution.

4. *Disagreements in attitude, like disagreements in belief, presuppose criteria of correctness.* The two sorts of disagreement in belief are the exact analogues of the two sorts of "ethical" disagreement. This can be shown by means of one of the examples Stevenson gives of ethical disagreement:

The trustees for the estate of a philanthropist have been instructed to forward any charitable cause that seems to them worthy. One suggests that they provide hospital facilities for the poor, the other that they endow universities. They accordingly raise the ethical question as to which cause, under the existing circumstances, is the more worthy. In this case we may naturally assume that the men are unselfish and farsighted, having attitudes that are usually referred to, with praise, as 'moral ideals' or 'altruistic aims' (p. 13).

Stevenson analyses this case as one that involves an initial disagreement in attitude (one man favours establishing hospitals, the other favours endowing universities) which will be resolved when both men share the same attitude:

Perhaps the men will disagree . . . about the present state of the poor, and the extent to which hospital facilities are already provided for them. Perhaps they will disagree about the financial state of the universities, or the effects of education on private and social life. . . . If the men come to agree in belief about all the factual matters they have considered, and if they continue to have divergent aims in spite of this—one still favouring the hospitals and the other the universities—they will still have an ethical issue that is unresolved. But if they come to agree, for instance, in favouring the universities—they will have brought their ethical issue to an end ; and this will be so even though various beliefs, such as those about certain social effects of education, still remain debatable (p. 14).

This way of putting the matter conceals the real nature of the disagreement between the trustees. Just as, when two people differ in opinion, the issue between them is, "Which opinion towards *p* is correct?" so, when the trustees disagree "as to which cause, under the existing circumstances, is the more worthy", the issue between them, if interpreted as a disagreement in attitude, is, "What attitude towards each of the alternatives is correct?" Now on the assumption that the trustees share the *same* moral ideals and altruistic aims and that they are unselfish and farsighted, their disagreement is *ex hypothesi* the analogue of (1a), above. For both of them regard the same sort of reasons as good reasons for favouring or disfavouring a proposed course of action. They mean the same thing by "worthy", in the sense that if both were agreed as to the consequences of providing hospital facilities for the poor and of endowing a university, they would agree on which of these alternatives is the more worthy, and therefore to be *correctly* favoured. Their disagreement in attitude is susceptible of being settled by rational means because (i) both appeal to the same norms when they try to decide whether providing hospital facilities or endowing a university is the more worthy, *i.e.* ought to be favoured by persons with their "moral ideals and altruistic aims"; and (ii) both, since they are "unselfish and farsighted", will presumably actually favour the alternative they believe is the more worthy.

We can turn this disagreement into the analogue of (2a), above, by negating the hypothesis concerning the trustees' moral ideals. Suppose that one of them is an altruist and the other a disciple of Nietzsche. Then the altruist might prefer providing hospital facilities for the poor to endowing a university, "because it would result in the greatest good for the greatest number", and the Nietzschean might favour endowing the university, "because men are not equal, and those intelligent enough to go to a university are intrinsically more important

than the poor". Neither would accept the reason of the other as a good reason for regarding his own attitude as incorrect.

→ In the light of these considerations, it is clear that with respect to what must be presupposed if the disagreement is to be susceptible of being settled by rational means, a disagreement in belief and a disagreement in attitude are not different, but alike. If this is so, Stevenson's conclusions concerning the manner in which disagreements are settled are on the one hand too sweeping, and on the other not sweeping enough. They are too sweeping because, when a disagreement in attitude arises between people who implicitly acknowledge the same standards of value, discussion and inquiry can in principle at least disclose whose attitude, if either's, is correct, as defined by the mutually accepted standards of correctness. And they are not sweeping enough because, when a disagreement in belief arises between people who do not ultimately abide by the same rules for acquiring and rectifying beliefs, rhetoric, and not logic, will settle their disagreement, if it is to be settled by discussion at all. In a disagreement of this kind, the statement, "You are unscientific", is no less emotive than, in its counterpart in morals, is the statement, "You are immoral".

The analogy between disagreements in attitude and disagreements in belief can profitably be pushed further. Let us notice that a disagreement in belief is not a logical relation between *propositions*, such as the relation of contradiction between *p* and not-*p*. It is a relation between epistemic attitudes; a disagreement in belief is a difference of opinion. (I use "opinion" to refer to, indifferently, beliefs or disbeliefs.) Nor is the disagreement, in Stevenson's sense, the relation that may obtain between opinions and facts, by virtue of which whenever an opinion is in disagreement (in some sense) with a fact it is erroneous. It is, once more, a relation between opinions, and it may hold as well between two erroneous opinions as between one that is erroneous and one that is sound. Similarly, two opinions may be in agreement with each other, yet both be erroneous, because neither "agrees" with fact. It follows that when A and B settle a difference of opinion, in the sense that whereas prior to discussion A believed *p* and B disbelieved *p*, after discussion they both believe *p*, their agreement in belief is not a sufficient criterion that they know *p* to be true. For *p* may be false.

Let us now ask: What is the aim of inquiry? Shall we take Peirce's dictum literally and say that "the sole object of inquiry is the settlement of opinion"? Or shall we qualify it, as Peirce

himself did, so that the aim of inquiry (as opposed to that of persuasion, for example) is conceived to be that of bringing about agreement between our opinions and facts? The latter alternative is the one we adopt. The aim of inquiry is to find the correct opinion. As an inquirer, I desire to know; my essential aim is to bring my opinion into agreement with fact, not with the opinion of anyone else.

But none of us is merely an inquirer. We are also social animals, who want others to share our knowledge, and who dislike having others disagree with us, not only on important matters, but sometimes on trivial ones as well. Besides, disagreements in belief may be the basis of disagreements about practice. Accordingly, when two people have a difference of opinion, they as often as not will try to eliminate their disagreement. There are all kinds of ways in which this might be attempted. For instance, A might hypnotize B, and suggest that B share his opinion. Or, A might administer a belief-inducing drug to B, as when insulin is given to a man who is believed to be really John Doe, but who believes himself to be Napoleon. Or, lacking hypnotic powers or appropriate drugs, A might try to change B's opinion by eloquence, or by exciting his passions. None of these methods is a rational method for settling a difference of opinion, however effective it may be in causing agreement of opinion, because in none of them is the cause of B's belief a reason for his belief. However, the fact that such methods often are effective in inducing belief, whereas the methods we call scientific are often ineffective, sometimes even when used upon scientists, permits us to conclude that the relation between the epistemic attitude of belief and its supporting reasons is not logical, but psychological, in precisely the same sense that, according to Stevenson, the relation between reasons and attitudes is not logical, but psychological. It does not, however, permit us to conclude that there is no *difference* between a mere cause of belief and a reason for belief, nor between a rational man, whose beliefs are caused by his consideration of reasons, and an irrational man, whose beliefs are determined not by consideration of reasons, but by other causes. Nor, on analogous grounds, can we conclude that there is no difference between mere causes of attitudes and reasons for attitudes, and between wise men and fools.

5. *Stevenson's first pattern of analysis is defective because it arbitrarily ignores the distinctions just made.* Stevenson does not doubt the validity of the distinction between rational and irrational beliefs. But despite his claim that he seeks to free the emotive theory "from any seeming cynicism", the effect of his

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analysis is to dissolve the analogous distinction between rational and irrational attitudes. In the many cases of ethical disagreement which he discusses, the disputants never attempt to discover whose attitude, if either's, is correct, so that their disagreement can be settled in the sense that both parties come to share an attitude they judge to be correct. Rather, whatever procedures they use to resolve their disagreement are interpreted as being, if not conscious, then unconscious, efforts merely to *change*, not *rectify*, attitudes. On Stevenson's view, an ethical disagreement is "settled" if and when the disputants come to agree in attitude, and there is no consideration whatever given to the possibility that their attitudes may nonetheless be incorrect, in the obvious sense in which people are said to have incorrectly favoured something when they have purchased inferior merchandise, entered upon unrewarding careers, or made unhappy marriages. On the contrary, one of the paradoxes of Stevenson's theory is that attitudes are never, even in this simple sense, incorrect.

This is perfectly clear on the first pattern of analysis. If at time t_1 A asserts, "X is good", and at a later time t_2 , as a consequence of discussion and inquiry, he asserts, "No, X is bad", he has not discovered reasons for supposing himself to have been mistaken at t_1 . For, on the first pattern, at t_1 A asserted, "I (now) approve of X", then was caused to undergo a change in attitude, and at t_2 asserted, "I (now) disapprove of X", and the judgment made at t_2 does not contradict the judgment made at t_1 . In commenting on this analysis, G. E. Moore wrote that he thinks Stevenson's view *may* be true, and that he has some inclination to think that it *is* true. "And, going far beyond Mr. Stevenson's cautious assertion, I have a very strong inclination to think that, *if* there is at least *one* 'typically ethical' sense of which these things are true, then of *all* 'typically ethical' senses these things are true."¹

I submit that if one reflects on the concrete cases to which this analysis allegedly applies, it is plainly seen to be false. Suppose that X is a bottle of wine, which at t_1 I judge to be good and purchase. In the evening I taste it and discover that it is sour. As a result, at t_2 I judge that the wine is bad. This judgment is not merely "different" from my judgment at t_1 ; it is the *correct* judgment as to the value of the wine. And my tasting the wine has not merely "altered" my attitude toward the wine; it has *rectified* it. This seems to me so very plain that

¹ *The Philosophy of G. E. Moore*, edited by Paul Arthur Schilpp, pp. 544-545.

I wonder why it is ever denied. Perhaps one reason is that those who do deny it think that the possibility of my making correct or incorrect judgments as to the value of the wine, and of having correct or incorrect attitudes towards it, presupposes that my taste in wine is universally shared. It does not. My evaluation of, and attitudes towards, wine are correct or incorrect relatively to my standards of the value of wine. If my standards are not the *same* as someone else's, this does not mean that I have *no* standards, so that whatever I judge good *is* good, or that whatever I favour I *ought* to favour. If, when I desired wine, I approved the purchase of what is really vinegar, there is no doubt whatever that I made a mistake. The fact that someone else might approve of my buying vinegar instead of wine, and that I may not be able to convince him that my misfortune *was* a misfortune, has no bearing on the question under discussion—whether value judgments can be correct and incorrect. That there are all sorts of difficulties which stand in the way of giving a satisfactory analysis of the sense in which the judgment that was made at t_1 is incorrect, I grant. However, that this judgment *is* incorrect, and *is* contradicted by the one made at t_2 , are data which, it seems to me, a theory of such judgments should account for, and not explain away as illusory.

If we try to remain as close as possible to Stevenson's view, and yet hold fast to data, a possible analysis is that when, at t_1 , I assert, "The wine is good", I am not merely expressing my approval of the liquid denoted by "wine", but am really formulating a *judgment* that the wine is actually of a kind I approve of. "Wine" here is used indicatively. It means "the liquid in this bottle", without implying either that it is spoiled or unspoiled. What I judge about the subject "wine" is that it has certain characters, among them that of being unspoiled, that I approve of. Since it does not really have these characters, my judgment that it does have them is incorrect.

To forestall the possible objection that my example of the wine is trivial or atypical, let us choose one of Stevenson's own. He writes :

Group II. In this group and those that follow, an ethical judgment is supported or attacked by reasons that are psychologically related to it. . . . They are simply of a sort that may lead one person or another to have altered attitudes in consequence of altered beliefs and so, thereafter, to make different ethical judgments.

(5) A : The proposed tax bill is on the whole very bad.

B : I know little about it, but have been inclined to favour it on the ground that higher taxes are preferable to further borrowing.

A: It provides for a sales-tax on a number of necessities, and reduces income-tax exemption to an incredibly low figure.

B: I had not realized that. I must study the bill, and perhaps I shall agree with you in opposing it.

A has supported his ethical judgment by pointing out to B the nature of that which is judged. Since B is predisposed to oppose anything of that nature, he shows his willingness to change his attitude, unless, perhaps, further study will disclose matters that weigh the balance to the other side. If B were not a person predisposed to disapprove of the provisions mentioned, however, he would find A's reasons unconvincing, and the argument would probably lead to a discussion of whether these provisions are good or bad (pp. 118-119).

The words "the proposed tax bill" and "it", as used by A and B, have on Stevenson's analysis a purely denotative function. They serve to orient both speakers toward the same object without specifying what sort of object it is. But it is clear that *what* A disapproves of when he disapproves of "the proposed tax bill" is "the bill which provides for a sales-tax on a number of necessities and reduces income-tax exemption to an incredibly low figure", and which, as Stevenson's amplifying example (6) shows, "will put a great burden on the poor, and make little difference to the rich" (p. 119). B does not favour *this* bill. "The proposed tax bill" he does approve of is "the bill which by providing for higher taxes would eliminate the need for further borrowing, and which does *not* do this by providing for a sales-tax etc." Stevenson contends that we must not say of such a case that the disagreement in attitude is "apparent" only, because if "the same X could be *recognized* by both parties regardless of their divergent beliefs about it, then the latter idiom (*i.e.* that the disagreement in attitude is only apparent) would be seriously misleading. One man was definitely striving for X, and the other definitely striving to oppose it; and if this involved ignorance, where one of the men was acting to defeat his broader aims, it remains altogether appropriate to say that the initial divergence in attitude, so far as X was concerned, was genuine" (p. 6).

Nonetheless, if B is predisposed to *disfavour* what "X" *actually* is, must it not be granted that his favouring *it*, when he mistakenly supposes himself to be favouring something different, is incorrect? And must it not also be granted that his judgment that the *actual* "X" is good is erroneous, as he discovers when he acquires correct beliefs about "X"? And further, must it not be granted that when A points out to B the actual nature of the proposed tax bill, he not only alters B's attitude but rectifies it, and that the reasons he gives to support his judgment that the bill is "on the whole very bad" are rightly accepted by

B, who by hypothesis is predisposed to favour the same sort of tax bills as A, as good reasons?

If the foregoing contentions are not admitted as being obvious, which they seem to me to be, and as they seem to the ordinary man who says, "All that glitters is not gold", I offer two arguments to support them. The first is to the effect that most, and I suspect all, of the cases classified in Chapter V of *Ethics and Language* as being cases in which value judgments are not logically supported by reasons are really of the same sort as those which are classified in "Group I", of which Stevenson says: "The examples in this group illustrate some of the ways in which ethical methods resemble factual ones. They present *exceptions* to the rough but useful rule mentioned previously—the rule that ethical judgments are supported or attacked by reasons related to them psychologically, rather than logically" (p. 115). The first two cases in this group are the following:

- (1) A: It would be a good thing to have a dole for the unemployed.
 B: But you have just said that a dole would weaken people's sense of independence, and you have admitted that *nothing* which has that consequence is good.

Here B attacks A's position by pointing out a formal inconsistency....

- (2) A: It is always wrong to break a promise.
 B: You speak without thinking. There are many cases of that sort which you regard without the least disapproval.

B's reply is an empirical assertion, but note that it contradicts A's judgment (by the first pattern only, of course) and so is logically related to it. A must, in the interest of consistency, either reject B's assertion or give up his ethical judgment (pp. 115-116).

The logic of the cases in this group can be made more clearly apparent by the following:

- A: I disapprove of X.
 B: No, you do not disapprove of X. X is of kind K, and you approve of whatever is of kind K.

It is identical with the logic of the case in which A and B disagree about the value of the tax bill:

- B: I approve of the proposed tax bill.
 A: No, you do not approve of it. It provides for a sales tax and reduces income tax exemptions, and if enacted it would put a burden on the poor and make little difference to the rich, of which you disapprove. So it is actually the kind of bill you disapprove of.

My second argument is to the effect that while cases (1) and (2) are genuine exceptions to the "rule" that value judgments are

not supported or attacked by reasons related to them logically, it is inconsistent of Stevenson to admit it. In case (2) Stevenson makes B understand A's assertion, "It is always wrong to break a promise", to mean, "I always disapprove of promise-breaking". But the correct first pattern analysis is, "I (now) disapprove of all promise-breaking". He says, "The tense of the verb indicates the time element of that which is judged, rather than that of the speaker's attitudes" (p. 93). (Cf. also pp. 165-168.) If what "X is good" really designates is "I (now) approve of X", then the only genuine exceptions to Stevenson's "rule" would be cases analysable as follows:

A (at time t_1): I now approve of X.

B (at time t_2): No. You did not approve of X at time t_1 .
Were you not guilty of faulty introspection?

I conclude that the descriptive meaning of statements of the form "X is good", where "X" stands for some actual object and not a concept, cannot be analysed into statements of the form "I approve of X". I suggest, without desiring to put forward an alternative theory of my own, that statements of this form can more plausibly be explained as statements which formulate judgments to the effect that X has certain characters p, q, r , by virtue of which it is approved of. This view at least puts us on the road to understanding how we can sometimes be mistaken in our evaluations, and in what sense we sometimes have inappropriate attitudes towards objects. If X does not really have certain characters, we are mistaken when we judge that it does have them. And if we approve or disapprove of X by virtue of our imputing to it characters it does not have, our attitude is incorrect, or at the most only accidentally correct, in the sense in which a mere belief as contrasted with knowledge might be accidentally correct. This view preserves, moreover, the distinction between wisdom and folly, the baby that Stevenson, in his anxiety to show that ethical disagreements of type (2) are irreconcilable by rational methods, threw out with the bath water.

6. *Both the first and second patterns of analysis seem plausible only if the difference between "statements" and "judgments" is ignored.* If it be objected that all that is proposed in my suggested analysis is provided for by Stevenson's second pattern of analysis, I reply: (a) If the second pattern really does differ from the first, Stevenson at any rate does not think so. He says, "It has been remarked that the second pattern differs from the first in its external aspects alone. The old factors have only to be recognized in their new form. In the present chapter this

contention will be established in detail, with particular attention to methodology. It will be shown that our previous conclusions, as developed for the first pattern, can be extended to the second without essential change" (p. 227).

(b) According to the second pattern (as well as the first), what is analysed is not a judgment but an *utterance*. My utterance "The wine is good", on the second pattern, designates that the wine has certain characters, and suggests, "I approve of the wine; do so likewise". What I contend is that the utterance formulates a judgment as to the value of the wine, just as the utterance "The wine is red" formulates a judgment as to the colour of the wine. The point to be emphasized is that "making a judgment" is not synonymous with "uttering a statement". A judgment, which is a mental act by which a predicate is ascribed to a subject, does not suggest anything to anybody, although the sentence which formulates the judgment may suggest a great deal. And one may judge that the wine is good, just as one may judge that the wine is red, without uttering the sentence which formulates the judgment, even to oneself. Furthermore, questions of intent to deceive and so on aside, one would not utter the sentence "The wine is good" unless one had judged that the wine is good.

(c) Judging the value of something, once the above distinction is made, does not consist in "recommending it for someone's approval or disapproval", nor of "encouraging, altering, or redirecting people's aims and conduct". We do these things not by making judgments but by making statements, assuming that we are limiting ourselves to merely verbal techniques. Now we would not make statements regarding the value of things unless we had first made the judgments. One recommends something that one has judged to be worthy of recommendation; and to judge something to be worthy of recommendation is not actually to recommend it. Once this distinction is granted, one of the main props for the emotive theory falls to the ground. For, it seems to me, it conclusively disposes of such arguments as Stevenson's argument from "ethical judgments and avoidability". This argument is as follows :

After saying, "In evaluating conduct people usually limit their judgments to actions which they consider *avoidable*, or subject to voluntary control" (p. 298), Stevenson asks, "Why is the statement, 'A's action was unavoidable', so frequently accepted as a reason for withdrawing an ethical judgment of A's action?" (pp. 301-302). His answer is that "our main purpose in judging an action is to control it, or to control a future one that is like it"

(p. 304). "We tell a man that he ought not to steal in order to keep him from stealing. . . . Our motive is much the same when we make ethical judgments of something which has already been done. If the man has stolen something, we tell him that he ought not to have done so. . . . We are trying to prevent similar actions in the future" (p. 302). Since only actions dependent on the agent's choice can be influenced in this way, we do not evaluate actions which are unavoidable, *i.e.* involuntary.

In short, for Stevenson moral judgments are instruments for rewarding and punishing agents, so as to encourage or discourage the occurrence of the types of actions judged, and they will fulfil their purpose only in so far as the actions are subject to the agent's control, and the agent himself is susceptible of being altered by them. If the actions are unavoidable, or if the agents are immune to criticism, the judgments are pointless, or merely retributive. Stevenson acknowledges the similarity between this view and the corrective theory of punishment. He says (his italics) that "although the relation between avoidability and *ethical judgments* has never (to the writer's knowledge) been analysed in quite the present way, a parallel analysis has repeatedly been given with regard to avoidability and *punishment*. Reformative and preventive theories have long made clear that punishment of unavoidable acts would fail to serve an important purpose. All that has been overlooked is that ethical judgments, being quasi-imperative, have also a reformative and preventive function. Theorists have been blinded to this by their almost incredible overemphasis on the cognitive aspects of language" (pp. 306-307).

I submit that the blindness of theorists to the quasi-imperative nature of ethical judgments may have been due to their observance of the distinction between judging the value of actions or agents and uttering the words which formulate judgments. We may judge that an agent who did A when, we believe, he could have done B had he chosen to do so, acted wrongly, *i.e.* chose the worse action instead of the better. We might never express the judgment, to the agent or to anyone else. However, if we wish to punish the agent, or to influence his future actions, and think that our words will be effective to this end, we may say to him, "You ought not to have done A". If he should then show us that his action was unavoidable, we would revise our judgment that he had acted wrongly, not because it can have no influence over his future conduct, but because we have learned that it is erroneous. The statement, "A was unavoidable", formulates a genuine reason for rejecting the judgment, "A was (morally) wrong",

because the predicate "(morally) wrong" cannot congruously be applied to involuntary acts.

Consider another of Stevenson's examples :

A : You ought to give the speech as you promised.

B : That is unfortunately beyond my power. My health will not permit it.

This example deals with the consequences of a judgment's *influence*. A is endeavouring to influence B to give the speech. If B's reply is true, then whatever influence A's judgment may have on attitudes, it will not have the further consequence of making B speak. Realizing this, A will be likely to withdraw his judgment; he sees that it cannot have its intended effect (p. 126).

What are we to say of this commentary ? I venture to observe only this : If A revises his *judgment* that it is B's duty to give the speech, it is not because he realizes that it is useless to try to make B speak, but because he realizes that it is not B's duty to give the speech. Presumably, B did not promise, "I will speak even though my health will not permit it".

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V.—ORDINARY LANGUAGE AND PROCRUSTEAN BEDS

BY C. D. ROLLINS

PHILOSOPHERS often assume that to depart from ordinary usage without notice is to violate it. They often assume, I mean to say, that anything which we might with any excuse call a "departure" from ordinary usage, and which is not at the same time justified by a new definition, must be a "misuse of language" and a linguistic fault.

I wish here to examine this assumption; and I shall do so in terms of a famous assertion by Professor G. E. Moore, and a criticism thereof by Professor Norman Malcolm. The assertion of Moore's, which I shall take as a test case, is the one which he made before the British Academy in 1939 when he said, "I do know that I held up two hands above this desk not very long ago". The criticism by Malcolm which I shall consider, is his recent one that Moore's assertion, far from defending common sense, was a "radical departure from ordinary and correct usage", a "misuse of language", "without sense".¹

I am concerned with Malcolm's criticism because it is one of the most clearly argued applications of the above assumption which I think may be found in recent literature. I am concerned to show, however, that Moore's assertion survives the criticism, and therein provides an illustration even though an unwitting one, of how wrong is the above assumption as a guiding principle in philosophy.

MALCOLM'S ARGUMENT

Malcolm's argument goes essentially as follows:

Ordinarily a person says 'I know that . . .' only when (1) there is or has been a question at issue and a doubt to be removed, when (2) he is able to give a reason for his assertion, and when (3) there is an investigation which, if it were carried out, would settle the question or doubt. But all these features are missing

¹ For Moore's assertion, or set of similar assertions, see "Proof of an External World", *Proceedings of the British Academy*, 1939, esp. p. 298. For Malcolm's criticism see "Defending Common Sense", *Philosophical Review*, May, 1949, pp. 201-220.

from Moore's assertion; for in his philosophical context he precluded them intentionally. If anyone had at the time expressed any doubt or question as to whether Moore was indeed holding up his hands, or again if there had been the possibility of Moore's giving a reason for his assertion, or if any investigation, like looking more closely at the hands, had been proposed as natural or appropriate, Moore would have changed his example of what he claimed to know. In his case we can allow that there was at most a *philosophical* question or doubt, but not a question or doubt; a *philosophical* reason but not a reason; a *philosophical* investigation but not an investigation. Thus Moore's use of 'know' was governed by conditions other than those in which we attach sense to the word. His use was a misuse of language and without sense—a radical and enormous departure from ordinary and correct usage.

Now it can hardly be denied that in this argument occurs the assumption in which we are interested. For without it the very thread of the argument would be lacking. Plainly it is assumed that, if Moore's use of 'know' was not in all logical features strictly the *same* as that familiar or most ordinary usage with which he was concerned—if his use was at all *different* and so with some excuse could be said to "depart" from that familiar usage—then, since it was not a case in which 'know' was being given a *new* sense, it must therefore have been a case of a *misuse*, or of a use *without* sense.

This assumption, as I shall now try to prove, is a mistake. We are indeed forewarned against it by the well-known fact that very often when we find an expression being used, even without warning, in a sense slightly different from the most ordinary, we do *not* regard it as a misuse or fail to understand it or to agree about its meaning. Hence it would appear that the assumption which occurs in Malcolm's argument is actually a faulty generalization. It would appear to have arisen from those many cases in grammar and in philosophy in which to show some difference in sense happens to be sufficient, elliptically, to show a misuse. Such cases are undeniably frequent, and their frequency is sometimes worth emphasizing; but surely not at the cost of a false generalization! It might here be worth emphasizing, on the contrary, that there tends to be an unholy prestige about such critical expressions as 'misuse'—partly, I suppose, because we originally encountered them in school where their use by the schoolmaster was authoritative, so that they tend to be curiously easier to affirm than to deny.

The above assumption is a mistake, however, not simply because there are such things as ordinary metaphors and simple extensions (by analogy) of a word's application. It is a mistake also because there are deviations from the most normal use which are *not extensions nor metaphors* in any ordinary sense; which are more like idiomatic definitional uses because they *deviate in order to represent the normal use faithfully in idiomatic but logically unusual contexts*: and which, while not being strictly the same as the most normal use, are yet only misleadingly called 'departures' since they are not violations but rather adherences.

But since nothing is better than a concrete example, I wish to turn at once to Moore's actual assertion, "I *do* know that I held up two hands above this desk not very long ago".

REVIEW OF MOORE'S ASSERTION

In reconsidering this assertion, the conspicuous fact which must be accounted for is the fact that it was generally received, and has generally been understood, as a very effective one indeed. Far from making his hearers and readers feel that he was misusing language or making an utterance without sense which departed radically from ordinary and correct usage, it had on the contrary the general effect of emphasizing, perhaps more vividly than had yet been done, that there was something undeniably confused and absurd in the sceptical view. Though this effect was achieved informally, it was none the less achieved; and we plainly could not call it a matter of mere persuasion or of verbal coercion. The assertion had point and unmistakable relevance; and no more here than elsewhere can informality be taken as a sign of nonsense. In short, not only did Moore plainly *intend* to be using 'know' without violating ordinary usage, but plainly also he has generally been understood as having succeeded. Even if we could point to no more than the historical fact that people have generally given his assertion this kind of reception, this fact in itself might be of great logical importance in judging whether he had used language properly. For in questions of usage, general opinion is often crucially relevant as it could never be, for example, in physical science.

The question which thus naturally arises is whether a use of 'know' which so effectively defended the most ordinary usage could rightly be called a "misuse"—whether a use which so

effectively defended "correct" usage could itself be rightly called "incorrect". Commonsensically we should be inclined to say that it could not. Commonsensically, it would seem that Moore's use of 'know' had *some* sense, *some* kind of function even though not strictly the same as the most ordinary.

What kind of sense or function, then, can it be said to have had? I think the answer to this question must be that it had a hybrid kind of sense, semi-assertional and semi-definitional, with which we are acquainted in everyday life; which is not neatly describable in terms of the usual simplified categories of logicians, but which can be recognized in Moore's case as genuine, and certainly as being a matter neither of misuse nor of nonsense. The nature of this sense or function I shall now attempt to describe. But I must insist that even should my description be regarded as unsatisfactory, this would not mean that Moore was using 'know' without sense. For often we understand expressions without being able to give perfect descriptions of their uses.

Moore's assertion, it will be remembered, was directed against philosophers who denied that we could ever know that there are external objects. He assumed that by external objects they could only mean such things as hands and so on, for otherwise what would "external objects" be, and why else should these philosophers argue as though they were *correcting* anyone, or saying something understandable? It may now seem to us that in this point Moore somewhat underestimated the sceptics' view (as well as Kant's worry) and that, despite remarks by Hume, Bradley, McTaggart, Russell, Broad and others, he ought not to have tried dealing with doubt of the external world as though it were always simply like doubt of the existence of thousand-pound notes or of gorgons and harpies. Yet, selecting his target as he did, and assuming that hands *were* examples of external objects, Moore proceeded in effect to dramatize a reminder about how an expression like 'hand' is actually and correctly used, and less directly, a reminder also about how we actually and correctly use 'know'. He might be described as doing something akin to ordinary ostensive definition, although because the sceptics were already acquainted with the expressions whose usage was being demonstrated, it was more like a re-enactment of what might *once* have been an ostensive definition, direct in the case of 'hand' and indirect in the case of 'know'.

Now in offering this description of what Moore was doing, I wish to guard against several misunderstandings. I am not suggesting that he would himself have given such a description as

this, or would necessarily agree to it at present; I am merely trying to account for the effect which his assertion had. Especially I am not suggesting that he could have given an ostensive definition for 'know' in anything like the clear and simple and direct way in which ostensive definition is possible for 'hand'. I merely mean, when I speak of something akin to ostensive definition, to stress that Moore's holding up his hands had unmistakable *relevance* to the assertion in which he uttered 'know'; and I merely take advantage of the fact that *no* ostensive definition is completely without implicit linguistic background which might be very complicated were we to attempt describing it exactly. Another thing which I am not suggesting is that, in giving something *like* a definition (or a usage-portrayal) Moore was exactly instancing any regular or stock variety of thing for which logicians have a name; for only the simplest kinds of usage—only those which can most easily be recognized in a simple or an exact picture—come to acquire recognized names. And finally there are two further misunderstandings which I wish to guard against: I do not mean to deny that Moore might have answered various questions at the time unsatisfactorily (*e.g.*, "Are you intending to use 'know' strictly in its ordinary way?"); and I do not deny that there was *some* misleadingness in his procedure, and that conceivably he could, for example, have said more accurately something like this: "It is correct to assert 'I know that here is a hand' in circumstances which are like the present one in the following respects . . . but different in there having first been a plain doubt, question, etc."

All I am contending is that it is grotesque to call Moore's use a "misuse", or to say that it was "without sense", or that it "departed" from established usage as though it violated it. The kind of thing Moore was doing simply is not describable in such a way. Consider, after all, the way we speak in similar cases in daily affairs. Suppose, for example, that Jones does not know how a bicycle tyre-lever is used, and that you are to see that he learns. Instead of sitting in an armchair and telling him verbally how it is used, and also instead of arranging for him to witness your actual use of it in removing a tyre-casing and repairing a puncture, you might perfectly well do the trick for him merely by going through some of the right motions, though you remove no casing and there is no puncture. You have not *told* him how to use it, and you have not strictly *followed* that use; yet would we say that you had *misused* the lever? Or take a closer example (which like the previous one I owe to Professor

G. Ryle): you might either teach Jones how to conduct a burial ceremony, or correct him when he looks like doing it wrongly, simply by saying the right words with the right gestures though you do so with no corpse and with no grave. Or take still a closer example: if you were a signalman in a rail yard, with the job of giving confirming signals by a flag of colour A and disconfirming signals by a flag of colour B, and if over a cup of tea something were said which if it were not a joke implied that you never had occasion to use an A flag, you might perfectly well reply by waving your A flag in the well-known way although no signalling was then to be done, and you were on a dead track, and there was no observer on duty down the line. Clearly this range of examples could be extended until it shaded into Moore's kind of procedure, without loss of those features which authorize us to say, in accord with ordinary usage, that although the use of a tool or formula or flag or word is not strictly being described, and although the thing is not strictly being given the use which is to be conveyed, yet it is not being misused, nor is it stage property in a pointless action or in the doing of nothing. Moore's case is thus merely a sophisticated member of a class of cases in which it is plainly incorrect to say 'misuse' or 'without sense'.

I think that in Moore's case the question, indeed, would never have arisen did we not sometimes tend to be influenced by overly simple models of how language must be used. Influenced by models, and not attending to the conditions in which they are designed and most used, we easily tend to have a far too pedantic notion of what understood and therefore legitimate usage must be. For example when an expression is to be employed in a use which is logically related to some familiar use, and when yet it is not to be employed in some simple metaphorical or extended way—nor is some *other* familiar use for the sake of contrast—we tend to have the idea that there are only three possible alternatives: either its use must be strictly the same as that familiar use, or else it must be quoted, or else it can only be misused. But of course this is wrong. For is a *definition* a misuse? And must a definition, in ordinary discourse, contain a quotation? Yet can a definition be said to *employ* the expression in its defined use? Without even considering ostensive definition and I daresay a great number of distinguishable other kinds of use (more or less like definition, and less or more like assertion), it is clear that any such simple notion is pedantic. In any case there has not yet appeared any important reason why Moore's use of 'know' should be called a misuse or, in any realistic sense, improper.

MALCOLM'S MISUSE OF 'DOUBT'

Malcolm intends, of course, to provide against this kind of objection. His claim is that Moore's assertions presumed a context of *philosophical* doubt, but not of doubt.

But this is unsatisfactory. For one thing, it gives a vastly oversimplified picture of Moore's situation. I have been pointing out that Moore was faced with a *mixture* of philosophical doubt and plain doubt; that at least part of the plain doubt was on facts of usage; and that part was naive, *i.e.*, was doubt about objects. And I have been pointing out that by uttering the disputed expressions in a tailor-made circumstance, in which the object-doubt was clearly precluded by the circumstance itself, Moore was displaying this doubt, induced by the sceptics' view, as being quite erroneous. If what I have argued is substantially correct, then it cannot be right to speak as though there were no element of plain doubt at all in Moore's philosophical situation. It was this element which made Moore's assertions natural in the respects in which Malcolm regards them as unnatural.¹

Yet apart from this over-simplified view of Moore's situation, there is still a much more serious error. Malcolm's claim is as follows:

- (a) Moore's use had sense only if it was in a context of doubt.
- (b) Philosophical doubt is not doubt.
- (c) Therefore Moore's use was without sense.

Now we might all agree to (a). But we cannot agree to (b), nor grant (c) on any such grounds. It seems to me quite clear that Malcolm here violates established usage.

When would we understandably say that philosophical doubt is not doubt? It might be in the kind of circumstance in which we would say that inductive reasons aren't reasons, or that cats aren't animals. For in this kind of circumstance we would be using a familiar device (humorous often but not always) for citing a difference, important in the context, between some members of a class and the other members of the class, and for stressing that

¹ Max Black ("On Speaking With the Vulgar", *Philosophical Review*, November, 1949, p. 620) suggests that Moore at the outset could have put his assertion (or one like it) beyond the reach of Malcolm's kind of criticism, by introducing the question of what sort of object an assistant would later hold up, subsequently having the assistant hold up his *own* hand, and thence proceeding in much the same manner as now recorded. But this would have weakened Moore's assertion against the sceptics by stressing the importance of the particular word 'hand'. More than this, it would concede the criticism, and thus obscure the mistake.

something which is true of the latter members is not true of the former.

But it is not in this manner, nor in any legitimate other so far as I can see, that Malcolm distinguishes between philosophical doubt and doubt. For he intends unjokingly to commit us to a result which, as I have pointed out, is grotesque. Do we not often say, inquire, agree, disagree, prove or disprove that an assertion in answer to a philosophical doubt has sense? Malcolm argues that philosophical doubt is no more doubt than a rhetorical question is a question. But this does not help him. For by his definition, many doubts would be philosophical, the answers to which would be far more informative than any answer to a rhetorical question could ever be (for the answer is always obvious in the question, this of course being the whole *point* of a rhetorical question).

It might conceivably be objected in Malcolm's behalf that established usage in this particular instance suffers from confusion and that it ought not to allow us, as it does, to connect 'philosophical doubt' with 'doubt' and with 'sense'. But in the first place, this objection would not in any apparent way be a help to Malcolm, for if established usage is not the same as what he calls "ordinary usage", which he takes as correct, then what could "ordinary usage" be? And in the second place, would not the objection anyway be absurd? For how could a linguistic practice be confused unless it involved an error in every case of its occurrence? And how could this possibly be shown concerning an *established* linguistic practice? For an *established* linguistic practice is one which we know to be taught, so to speak, and learned, and used accordingly, and which must therefore have at least *some* occurrences in which it is free from error just as any definition by its own authority must be, and just as any instance of use must be, which conforms to the definition. I cannot here go into a proof of this general point, nor do I need to, since its essentials are already well known (one version of them was given by Malcolm in his essay "Moore and Ordinary Language" in *The Philosophy of G. E. Moore*). And quite apart from the general principle, I should think it now would be plain, in the present case, that there need be no error in saying that Moore was answering a doubt and that his utterance had sense.

CONCLUSION

In this paper I have readily granted that Malcolm has shown Moore's use of 'know' to have been not strictly the same, in all logical respects, as that familiar usage with which Moore was obviously concerned, and that to this degree Moore's assertion could with *some* excuse be called a "departure" from that usage. I have granted also that Moore was not giving a *new* sense to 'know' by any redefinition. But I have denied that his use was a "departure" in the manner of a misuse, a violation, or an employment of the word without sense. Thus I have in effect argued that the word 'departure' is easily misused and that Malcolm has genuinely misused it. My argument has been in accordance with the principle that in understood discourse a particular occurrence of an expression may have sense even though its sense is not neatly describable (for there may be complexity, where there is no confusion); and I have tried to make clear that Malcolm has overlooked the function and sense which Moore's use of 'know' actually had for many hearers and readers. Incidentally I have criticized Malcolm's assumption that doubt excludes philosophical doubt, and criticized his misuse of 'doubt' and his resultant misuse of 'sense' and 'misuse'. I have offered proof that Malcolm's use of these words is what we may *truly* describe as a misuse.

If my argument has been substantially correct, its general importance would lie in its tending to show, by a concrete example, that not only logicians who have been unhappy about ordinary language, but also some of their critics who have meant to defend it, have sometimes shared the fault of supposing that legitimate usage has not been legitimate, that understood usage has not been understood, where it did not satisfy pedantically simple criteria. Constructively, I have been suggesting that there are well-understood kinds of expressional occurrences which are neither simply assertional nor simply definitional, whether formally or ostensively; and that some of these uses deviate from the most normal usage in a way which is logically required by unusual yet idiomatic contexts—so that the deviation, while not being strictly the *same* as the most normal usage, yet *depends* upon it, is *determined* by it, faithfully *represents* it, and is the form which it *appropriately takes*, so to speak. The test for whether it does this, I have suggested, is whether and how it is generally understood, not whether it answers to a model or criterion which has become useful because it is simple and most frequently applicable. Thus, I have

suggested, to show as Malcolm does that a certain use, in a context admitted to be unusual, is *different* from the most normal usage, does nothing toward showing that it is a misuse or is nonsensical.

The objection to this view is likely to be that it is not sufficiently clear. But here it seems to me, the word 'clear' would mean 'simple'. And to object that the above view is not *simple* is no objection, for the view is offered precisely where expecting complex sense to be simple has led to a mistake. If we wish to discuss only *simple* kinds of language-use, then it is important to confine ourselves to those cases which really *are* simple.

This view is made particularly relevant to Malcolm's criticism by the fact that he intends to adhere to ordinary language. If he were intending merely to be showing the inexplicability of Moore's assertion in terms of certain simple criteria, there might be nothing wrong. But he intends to carry on philosophical analysis by using expressions like 'departure', 'misuse', 'sense' and 'doubt' in their most expected and ordinary import; and by this standard he has gone wrong in a way which can itself be shown in ordinary language.

Finally, in a case like Moore's, I grant that to say 'senseless' and 'misuse of language' might sometimes, in some contexts be rhetorically useful. It could be an impressive way of saying, for example, the kind of thing Malcolm has actually proven, namely that a certain use of a word, though it may idiomatically defend, represent and adhere to a familiar usage, is not itself strictly the same as that usage in all logical respects. But surely this can be said less misleadingly than in the terminology which Malcolm has employed. If anyone still wishes to speak in such a manner, I take it that we may at least refuse to accept the intimation that a procedure like Moore's ought to be avoided in philosophy. If this intimation be repudiated but the terminology persisted in, we shall not then be so concerned. For we are concerned less to agree in our final verbal inclinations, than to see upon what they are based.

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VI.—THE EMOTIVE THEORY AND RATIONAL METHODS IN MORAL CONTROVERSY

BY ASHER MOORE

NOT the least interesting fact about the so-called "emotive theory" of ethical sentences is the bitterness of the emotions it has aroused. I fear that, for some philosophers, even Professor Stevenson's scrupulously unemotional exposition of this theory¹ has had primarily an "emotive" meaning. Indeed, it sometimes seems that Stevenson is coming to represent, in the minds of some twentieth century philosophers, what Hobbes represented to the seventeenth and eighteenth centuries: a sort of Anti-Christ. I do not know whether Stevenson regards this as a compliment, or not.

A large part of the furor is due, I believe, to its being thought that the emotive theory has certain very radical implications with regard to some of the problems of ethics. These implications are supposed to be of a radically subjectivistic or irrationalistic sort; and they are received with joy or with thinly-veiled venom, depending on the ethical views of the philosopher concerned. I am even inclined to think that Stevenson himself is sometimes disposed to take some such view as this as to the consequences of his theory. It is true that he takes pains to call attention to the relatively more "respectable" aspects of the theory, and to make it as fit as he can for the ears of the young. But the very fact that he doth protest so strongly the virginal innocence of his theory only strengthens my suspicion that he really regards it as being, when all is said and done, pretty bold and saucy.

In this article I shall be concerned solely with Professor Stevenson's version of the emotive theory. And it will be my contention that the emotive theory—in the form in which it is held by Professor Stevenson—does not in fact have the radical and far-reaching consequences for ethics that it is often supposed to have. And I shall try to show that such consequences as it does have for ethics are so obviously true and so obviously harmless that they would gladly be accepted by any philosopher

¹ See especially his *Ethics and Language* (New Haven: Yale University Press, 1944) and his "Meaning: Descriptive and Emotive" in the *Philosophical Review*, v. 57, 1948, p. 127. All quotations from Stevenson are from the former work and are used with the permission of the publishers.

whatever, regardless of his particular ethical views. I do not doubt that Stevenson's analysis of the meaning of the term 'meaning' may have far-reaching consequences for *semantics*. But that is a different question. I am concerned only with the consequences his theory has for ethics.

The central questions of traditional ethical speculation can be divided into two classes :

First, there are questions concerned with the nature of goodness, rightness, duty, etc. These are questions as to the correct analysis of the meanings of such ethical terms as 'good', 'right', and 'duty.' And the sense of the term 'meaning' that is relevant when we say that traditional ethics has been concerned with the meaning of ethical terms is the sense that Stevenson calls 'descriptive meaning,' and that some other philosophers have called 'cognitive meaning'—not the sense which Stevenson calls 'emotive meaning.' Traditional ethics has been concerned, that is, with making explicit what *beliefs* we are holding about something when we apply an ethical term to that something.

Now I think it would be admitted by everyone, and is explicitly urged by Stevenson, that the emotive theory itself has no implications whatever as to the proper analysis of the descriptive meanings of ethical terms. In *Ethics and Language*, various traditional analyses are considered, and it is urged, correctly in my opinion, that the emotive theory is compatible with any of them. The general form of analysis of the so-called "Second Pattern", for example, is sufficiently broad to accommodate all of the traditional objectivistic analyses—whether these be of the "intrinsic quality" variety, the "consequences" variety, or the "good will" variety.

The *second* class of traditionally ethical questions contains those having to do with methodology. These are questions as to the proper way of establishing the truth or falsehood of an ethical judgment and as to the type of evidence which is relevant. And it is with regard to questions of this sort, I think, that the emotive theory is assumed to have very radical and far-reaching consequences. For the emotive theory holds that attitudes and feelings, as well as beliefs about matters of fact, are essential ingredients of any serious moral controversy. And it is assumed to follow from this that there can be no "rational" way—purely by the citing of evidence—to resolve moral controversies one way or the other ; and that hence moral arguments are not so much attempts to arrive at *true beliefs* as they are attempts to *influence feelings*.

Now I do not believe that any philosopher would seriously

deny that feelings and attitudes are involved in the total psychological complex surrounding a serious moral disagreement ; or would deny that at least a part of the aim of each of the parties to the disagreement is to bring the attitudes of the other parties into accord with his own. One might almost define a "serious" moral dispute as one in which an attempt is being made to get certain people to do certain things ; or, if not actually to do them, at least to be favourably disposed towards the doing of them. It was to this obvious fact, I take it, that Hume was pointing when he said : "Extinguish all the warm feelings and prepossessions in favour of virtue, and all disgust and aversion to vice : render men totally indifferent towards these distinctions ; and morality is no longer a practical study, nor has any tendency to regulate our lives and actions."¹ Nor, I should think, would anyone want to question Stevenson's assertion that it is at least possible that some of our differences of attitude do not depend wholly on differences of belief ; that a change of belief may be neither necessary nor sufficient to cause a change of attitude. Some philosophers would, to be sure, hold that all differences of specifically "moral" attitude do in fact depend on differences of belief—for example, Socrates ; some other philosophers would hold that all men without exception share in common some specifically moral attitude, so that disagreements in moral attitude never in fact occur—for example, Kant ; but I think anyone would grant the logical possibility that there are differences of moral attitude which are not founded on differences of belief. For it is surely not logically impossible that two people, given full and equal knowledge of a situation, should feel differently about that situation ; nor is it logically impossible that people's feelings should be subject to influence by methods which have nothing to do with proof or the giving of evidence. And this is all that Stevenson has claimed.

If everyone would admit these facts, then why is it that the emotive theory is felt to be peculiarly, and to an unusual degree, irrationalistic ?

I think the reason is that the emotive theory makes feelings and attitudes an intrinsic and inseparable part of the *meaning* of ethical sentences. On the more traditional view, the meaning of an ethical sentence consists wholly of the beliefs to which the sentence gives voice. Since these beliefs are all of them either true or false, the sentence expressing them must itself be either true or false. And this makes it at least possible that there is

¹ Hume : *Enquiry Concerning the Principles of Morals*, Sec. I.

some sort of evidence which could be given to show which it is—true, or false.

On the emotive theory, on the contrary, the meaning of an ethical sentence is not exhausted by the beliefs which it expresses. Included in its meaning also are the attitudes which it expresses. Since the very meaning of an ethical sentence thus includes non-cognitive elements—attitudes—the sentence cannot, except in a *Pickwickian* sense, be either true or false. And from this it clearly follows that there is no rational way of deciding whether the sentence is true or not, since, really, it hasn't got any truth-value at all.

Thus the difference between the traditional view and the emotive theory is held to be this: that on the traditional view, although there may be no rational methods of bringing everyone's attitudes into accord, there are rational methods of deciding whether what the participants to a moral dispute say is true or false; whereas on the emotive theory there are no rational methods of doing even this.

Now it is quite true that Stevenson holds that ethical sentences do have a sort of truth-value, and that he explicitly chides the positivists for maintaining that they do not. But it is also true that his whole case against the sufficiency of rational methods in moral controversy is based on his contention that a part of what an ethical sentence means has no truth-value. The descriptive part of the meaning can, he says, be true or false; and, in theory, rational methods are sufficient to determine which it is. But, in Stevenson's words, "the emotive meaning of an ethical judgment has nothing to do with truth or falsity." (P. 154.) Thus his view seems to be this: that although it may be convenient to speak of ethical sentences as having truth-value, since a part of their meaning is either true or false; it is also the case that a part of their meaning is not either true or false. And this assertion does have an "irrationalistic" ring to it. It does sound as if Stevenson were denying truth-value to something which some other philosophers had thought did have truth-value.

But let us examine the assertion, and see just how irrationalistic it really is.

I shall contend that—in the sense in which Stevenson uses the term 'meaning'—he is clearly and obviously right in holding that a part of the meaning of an ethical sentence admits of neither truth nor falsity. And that he would, indeed, be right if he were to make the further claim that—in this sense of the term 'meaning'—part of the meaning of every sentence, ethical or non-ethical, is neither true nor false. But I shall

contend also that—in Stevenson's sense of the term 'meaning'—the conclusion that a part of the meaning of an ethical sentence has no truth-value is perfectly innocent and harmless, and such as any philosopher would admit to be obviously true.

Let us take a typically ethical sentence—call it 'S'. And let us ask Stevenson: "What exactly it is that you believe S means? and what exactly is it that you are asserting when you say that the emotive part of what S means is neither true nor false?"

I may point out that I do not wish, in this paper, to raise any questions as to the usefulness or adequacy of Stevenson's definition of the term 'meaning'. It appears to me that it would no longer be cricket to criticize his definition of this term since he has in effect said, in an article in the *Philosophical Review*¹ that he wishes this definition to be accepted as a stipulation, regardless of whether or not it correctly analyzes the ordinary meaning of the term 'meaning'. I wish then to accept his stipulation that the term 'meaning' shall stand for the disposition of a sign to evoke certain sorts of psychological responses. And I wish further to accept his stipulations that this meaning shall be called 'descriptive' when the responses evoked are cognitions and shall be called 'emotive' when the responses evoked are attitudes.

We have asked Stevenson what it is that, according to him, S means. And we must now take notice of a rather odd fact. In ordinary language, the question 'What does S mean?' is equivalent to the question 'What is the meaning of S?' But strangely enough, Stevenson could not accept this equivalence, at least in any straightforward sense. For, if he did, it would turn out that none of what S means is either true or false. This is because a meaning, in Stevenson's sense, is a *disposition*—a tendency to cause in hearers various sorts of psychological reactions; just as the stimulating power of coffee, to use his own favourite analogy, is the tendency of coffee to cause accelerated responses. And it is clear that a disposition cannot intelligibly be said to have truth value, in any straightforward sense. Note also that if Stevenson were to admit the equivalence in question, it would turn out that nothing that a scientific statement means would have truth-value either; for its meaning too is simply a disposition.

Stevenson, then, will have to hold that the question 'What does S mean?' is not equivalent to the question 'What is the meaning

¹ See n. 1., p. 233.

of S?', but is rather equivalent to the question 'What psychological responses has S a disposition to cause?' And it now becomes clear that Stevenson would answer this question by saying that S has a disposition to cause (that is, in ordinary language, S means) both attitudes and beliefs. It is this answer which constitutes, I think, the distilled essence of the "emotive theory."

Let us now proceed to ask further whether what S means has truth-value. And we must not here be understood to be asking whether the *disposition* which, in Stevenson's language, is "the meaning" of S has truth-value, for the reason just given. We must be understood rather as asking whether the beliefs and attitudes which S has a disposition to arouse have truth-value. Now Stevenson acknowledges that whatever beliefs are called forth by S do have truth-value; this point we discussed previously. Hence, when he says that part of what S means has no truth-value, he must be taken as asserting simply and solely that the attitudes which S arouses are neither true nor false.

But this, I submit, is a patent and obvious fact. Surely we should all be prepared to admit that though beliefs may be true or false, feelings cannot intelligibly be said to be either. This is a fact which is compatible, it appears to me, with any ethics whatever, and one which would be admitted by even the most staunchly rationalistic philosopher. And yet this turns out to be all that Stevenson means when he says that the emotive part of the meaning of an ethical sentence is neither true nor false and hence not subject to rational proof or disproof.

The mistaken belief that Stevenson's theory is somehow peculiarly irrationalistic is due, I think, to two things. First, to the failure to notice the very peculiar sense in which—for better or for worse—Stevenson uses the term 'meaning'. And secondly, to the consequent failure to make sufficiently complex translations of his statements about emotive meaning into more ordinary English. When the proper translations are made, what he says turns out to be not at all irrationalistic, but obviously true, and such as would be admitted by everybody.

I shall give a few sample translations :

(1) Stevenson says that "the emotive meaning of an ethical judgment has nothing to do with truth or falsity." (p. 154.)

The proper translation into traditional English is: "the feelings which ethical sentences cause have no truth-value."

(2) Stevenson says that a person who admits that a given ethical sentence is true may nonetheless "reject" that statement.

The translation: A person may stubbornly refuse to have his feelings swayed by a sentence which he fully admits to be true.

(3) Stevenson says: "The effect of ethical terms in directing attitudes . . . must be explained with reference to a characteristic and subtle kind of *emotive meaning*. The emotive meaning of a word is the power that the word [has] . . . to evoke or directly express attitudes. . . ." (p. 33.)

The translation: The effect of ethical terms in directing attitudes must be explained in terms of the power ethical terms have to direct attitudes.

All of these assertions are, I should maintain, obviously true.

I think that Stevenson's basic contention on the question of the methodology of moral controversy may therefore be summed up in the following statement which he makes in *Ethics and Language* in connection with his criticism of the positivistic view that ethical sentences are neither true nor false. He says: "It is more accurate and illuminating to say that an ethical judgment *can* be true or false, but to point out that its descriptive truth may be insufficient to support [*i.e.* to cause] its emotional repercussions." (p. 267.) I cannot believe that anyone would want to deny this.

But perhaps someone, while agreeing to all of the above, will still maintain that Stevenson's theory is irrationalistic to a peculiar degree, and will give as his reason this: that Stevenson wants to include the whole psychological context of feelings and attitudes which surrounds an ethical dispute in ethics, instead of restricting 'ethics' to disagreements in belief.

Now it is certainly true that Stevenson does this. He uses such a term as 'ethical issue' in such a way that it is logically possible for there to be an ethical issue even when there is absolutely no difference of belief. He uses 'ethical argument' so as to include not only the rational process of trying to find out whose beliefs are true but also the whole process—partly rational, partly irrational—by which one party to the argument seeks to change the other's attitudes. And he uses 'ethical agreement' in such a way that there can be no ethical agreement between two people unless their moral feelings are in accord.

But this point—as to how much of the total complex present when two people have a moral disagreement we ought to include in ethics—seems to me to be completely trivial. So long as we are all agreed that the total psychological context involves both cognitive elements and emotional elements, and so long as we are agreed that the emotional elements may not be wholly dependent on the cognitive ones, it just doesn't seem to make any difference whether we decide to say that the "moral

disagreement" is solely one of belief—though with emotional "accompaniments"—or whether we decide to say, with Stevenson, that the emotional "accompaniments" are themselves a part of the moral disagreement.

William James once said that a theory goes through three stages: first it is attacked as absurd; then it is admitted to be true, but held to be obvious; and finally it is seen to be so important that its adversaries claim that they themselves discovered it. I find that I have adopted something very like the second attitude towards that part of the emotive theory which I have been discussing. For I have claimed that that part is not only true, but so obviously true that it is difficult for me to believe that any philosopher ever seriously disagreed with it. Perhaps I might say, therefore, that in calling Professor Stevenson's thesis 'obvious', I have been meaning to compliment him on that thesis. For in philosophy, it appears to me, to believe what is obviously true is a very great virtue.

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VII.—DISCUSSIONS

WHAT ACHILLES SAID TO THE TORTOISE

(Being a revised account of a famous interview, first reported in MIND in April, 1895, by Lewis Carroll.)

ACHILLES, as is well known, had overtaken the Tortoise, and had seated himself comfortably on its back, thus proving that he *could* overtake the Tortoise, although the race-course consisted of an infinite series of distances.

Whereupon the Tortoise turned mathematician once more and presented Achilles with a most perplexing problem in logic.

"Would you like to hear", he said, "of a race-course, that most people fancy they can get to the end of in two or three steps, while it *really* consists of an infinite number of distances, each one longer than the previous one?"

"Very much, indeed!" said the Grecian warrior, as he drew from his helmet an enormous note-book and a pencil. "Proceed! And speak *slowly*, please! *Short-hand* isn't invented yet!"

"That beautiful First Proposition of Euclid!" the Tortoise murmured dreamily. "You admire Euclid?"

"Passionately! So far, at least, as one *can* admire a treatise that won't be published for some centuries to come!"

"Well, now, let's take a little bit of the argument in that First Proposition—just *two* steps, and the conclusion drawn from them. Kindly enter them in your note-book. And in order to refer to them conveniently, let's call them A, B, and Z:

- (A) Things that are equal to the same are equal to each other.
- (B) The two sides of this triangle are things that are equal to the same.

Therefore

- (Z) the two sides of this triangle are equal to each other.

Readers of Euclid will grant, I suppose, that Z follows logically from A and B, so that anyone who accepts A and B as true, *must* accept Z as true?"

"Undoubtedly! The youngest child in a High School—as soon as High Schools are invented, which will not be till some two thousand years later—will grant *that*."

"And if some reader had *not* yet accepted A and B as true, he might still accept the *sequence* as a *valid* one, I suppose?"

"No doubt such a reader might exist. He might say 'I accept as true the hypothetical proposition that, *if* A and B be true, Z is true; but, I *don't* accept A and B as true'. Such a reader would do wisely in abandoning Euclid, and taking to football."

"And might there not *also* be some reader who would say 'I accept A and B as true, but I *don't* accept the hypothetical'?"

"Certainly there might. *He*, also, had better take to football."

"And *neither* of these readers", the Tortoise continued, "is *as yet* under any logical necessity to accept Z as true?"

"Quite so", Achilles assented.

"Well, now, I want you to consider *me* as a reader of the *second* kind, and to present me with premisses which will enable me to deduce the truth of Z."

"You want me to present you with premisses which will enable you to deduce the truth of Z?" Achilles said musingly. "And your present position is that you accept A and B, but you *don't* accept the hypothetical—"

"Let's call it C", said the Tortoise.

"But you *don't* accept

(C) If A and B are true, Z must be true."

"That is my present position", said the Tortoise.

"Then I must ask you to accept C."

"I'll do so", said the Tortoise, "as soon as you've entered it in that note-book of yours. Now write as I dictate:

(A) Things that are equal to the same are equal to each other.

(B) The two sides of this triangle are things that are equal to the same.

(C) If A and B are true, Z must be true.

Therefore

(Z) The two sides of this triangle are equal to each other."

"Now I see what you are driving at", said Achilles. "You want me to say that if one accepts A and B and C as true, then one *must* accept Z as true?"

"Exactly! That's what I wanted you to say."

"And then", continued Achilles, "you are going to tell me that this is *another* hypothetical—we'll call it D—and that since you don't accept *this* hypothetical, you will still be able to accept A and B and C as true, but *not* Z?"

"Precisely!"

"And then, no doubt, you will ask me to enter *this* hypothetical again among my other premisses, and say to you that, *if* you accept A and B and C and D as true, then you *must* accept Z as true?"

"I'll grant you; that *was* roughly what I expected."

"Well", said Achilles, "there's certainly a future for *this* kind of job! It will certainly go on until I shall have no more room in my note-book to write down the premisses! Indeed, I should have to remain seated on your back for all time, trying to find sufficient premisses to enable you to deduce the truth of Z!"

"Very true!" said the Tortoise triumphantly. "Very true, indeed! And now do you see that you have discovered what no logician is going to discover for centuries yet to come, an infinite

regress ?" (For the infinite regress had not yet *really* been invented).
 "And do you see", continued the Tortoise, "that no matter *how* long you try, you can never give me sufficient premisses to enable me to deduce the truth of Z ?"

"You do flatter me !" said Achilles. "I'll admit that I spotted the regress, but it was *you* who *invented* it."

"I *invented* it !" said the tortoise, with great astonishment. "One does not need to *invent* it ; it is just *there*, and one can't do anything about it."

"No", said Achilles, "I'll stick to my point. *You yourself* *invented* it. You invented it by compelling me to write down that condition C with the other two conditions A and B, and so deceiving yourself into thinking that C was on the same footing as A and B. Did you want to deceive yourself, or were you just trying to tease me ?"

"Neither, I assure you ! But where else *could* you have inserted C ? Can't you see that if my accepting the hypothetical is a *condition* of my accepting Z as true, it *must* be put in with the conditions A and B ?"

"Well, yes and no. It must be put in with A and B but not on the same footing as A and B. By making C into an *extra* premiss you are substituting, as Aristotle might some day put it, a hypothetical argument for a syllogistic argument. Instead of saying 'A and B, therefore Z', you *now* say 'If A and B then Z, A and B, therefore Z'."

"Why !" said the Tortoise, "even an intermediate student can see *that*. But this only brings in *another* hypothetical. And since I don't accept *this* hypothetical, I can *still* accept all the premisses and deny the conclusion. You still haven't presented me with sufficient premisses to enable me to deduce Z."

"Not so fast !" replied Achilles, somewhat impatiently. "The trouble with *you* is that you don't see that there are hypotheticals *and* hypotheticals. You've never distinguished, have you, between first and second order hypotheticals ?"

"When I did Honours", said the Tortoise, "there was certainly no news of them then."

"Well, then, I must briefly tell you about them. If I may use words which will only be understood in Oxford, and that only three thousand years hence, I would say that a second order hypothetical is a hypothetical which has another hypothetical—this time, a first order hypothetical—as its apodosis."

"Well, that is rather a mouthful ! Could you not say it, please, in ordinary standard English—which is not yet invented, of course, but which will be appreciated in Oxford three thousand years from now ?"

"By all means !" said Achilles, somewhat surprised to find so much humility and foresight all at once.

"A second order hypothetical", he continued, "states a condition

under which *another* hypothetical is true. A hypothetical about a hypothetical! A *two-storeyed* hypothetical!"

"And, I suppose, this kind of hypothetical is important?"

"Very important! Can't you see that in a second order hypothetical the condition is always a condition on which we can make a true conditional statement, and not, therefore, a condition which can itself be included in that conditional statement?"

"I see *that* most clearly", said the Tortoise, "but what *has* this got to do with our argument?"

"This, surely: that the statement of the implication in a so-called hypothetical argument must generally take the form of a second order hypothetical, and *not* a first order hypothetical as would be the case with a syllogistic argument."

"Could you give an example? We *must* have examples, you know!"

"Well, let me see", said Achilles, "instead of saying that, if one accepts A and B and C one must accept Z, we must now say that, if one accepts C, *then*, if one accepts A and B, one must accept Z."

"Sounds a bit odd, doesn't it?"

"Not really. If A and B together imply Z, then, if both A and B are true, Z is true. Is there anything odd about that? If so, you can say instead that, if A and B are true, Z is true—provided A and B imply Z. And there's nothing odd about that! *That's* just ordinary standard English!"

"And I am to take it that this *has* got something to do with our argument?"

"Of course! You look at it like this: by making C an extra premiss *and at the same time* making the statement of the new implication a first order hypothetical, you have assumed that the hypothetical C can be included in its *own* protasis!"

"I have said, so to speak, that if A and B are true and that if it is true that if A and B are true Z is true, then Z is true?"

"That's right. And by making C an *indispensable* premiss you have further assumed that this hypothetical cannot be true *unless* it is included in its own protasis!"

"But in *that* case, . . ." said the Tortoise.

"In *that* case", said Achilles, "the hypothetical in the protasis must also be true and included in its own protasis, and so on, *ad infinitum*. Which means that C can never be true, although you have already accepted it as true!"

"The point, I suppose", said the Tortoise, "is that no hypothetical can be included in its *own* protasis. I'll take my stand with you on *that*. But what must I then say? Must I then say that C *cannot* be an extra premiss?"

"Not exactly. The trouble is that this word 'premiss' is a bit ambiguous. It may mean a proposition which, singly or jointly with others of the same order, implies a conclusion, or it may mean a proposition which implies that some *other* proposition or propositions of a different order imply a conclusion."

"For convenience", said the Tortoise, "let's call the first a *premiss*, and the second a *meta-premiss*. This is going to mean generally, of course, that the major premiss in what generations of logicians are going to call a hypothetical argument, will not be a premiss but a meta-premiss—but I suppose that is by the way."

"Quite so", said Achilles. "The point now is that C cannot be an extra *premiss*, but if by a premiss you mean a *meta-premiss*, then you can have C, if you like, as a premiss."

"Most confusing!" said the Tortoise. "Tell me this: you distinguish between the *premisses* of an inference and the *principle* of an inference?"

"Most certainly! No reader of Joseph and Russell—once they have written on the matter, of course—can object to *that* distinction."

"Very well then", said the Tortoise. "Are you now saying that the premiss of an inference is a first order statement, while the principle of an inference is a higher order statement, a statement about certain *other* statements, a meta-statement?"

"I am saying *that* much, certainly. We must distinguish between an argument, which is an *operation* with certain statements, and the principle of an argument, which is a *statement* about that operation."

"And, I suppose, the principle of an argument cannot *itself* be included among those *other* statements which are asserted *in* that argument? It cannot be *asserted*, in the way in which those *other* statements are asserted?"

"That is right. At least, not without destroying the argument, or else creating a new *kind* of argument."

"Such *delightful* sophistry!" murmured the Tortoise. "If I take to football, *you* will have to take to professional philosophy!"

"Is that a kind of *game*?" enquired Achilles.

"Of course", said the Tortoise. "It's a game in which everybody confuses everybody else, and the winner is the one who confuses most, and all the participants are Players and not Gentlemen."

"Then I will be a *Gentleman*", said Achilles.

"In that case", said the Tortoise, "you must give a straight answer. Are you, or are you not, saying that the *principle* of an inference cannot be an additional *premiss* of that inference?"

"I am saying *that*", said Achilles, "and more. The principle of an inference cannot be an extra premiss of the *same* inference. But *that* doesn't mean that it cannot be an extra premiss. What it *does* mean is that if you make it an extra premiss, then the inference becomes a different *kind* of inference, your new premiss will be a different *kind* of premiss, namely a meta-premiss, and the principle of this new inference will be a different *kind* of principle, namely one which can be stated only by means of a second order hypothetical."

"Good!" said the Tortoise. "I will now have C as a meta-premiss, and I will have the new implication statement as a second order hypothetical."

"Excellent!" said Achilles. "You are now on the right road. Proceed!"

"Suppose", continued the Tortoise, "I *reject* this second order hypothetical. Will you not then have to put it also among the premisses—sorry, meta-premisses—and so generate third, fourth, fifth order hypotheticals, and so on, *ad infinitum*? If so, you still haven't presented me with sufficient premisses, that is, meta-premisses, to enable me to deduce the truth of Z."

"Now, my dear Tortoise", said Achilles, "I must ask you to notice just one further point. A meta-premiss differs from a premiss in one important respect, that only *one* meta-premiss is ever necessary in any argument. For if one such meta-premiss is true, the premisses referred to *in* that meta-premiss are in fact *sufficient* to imply a conclusion. Any additional meta-premisses therefore are *quite* superfluous."

"Either C is *not* a premiss", said the Tortoise, recapitulating, "or if it *is* a premiss it is a meta-premiss, in which case *one* such premiss is sufficient in any argument. . . ."

"That is so", said Achilles, putting his note-book and his pencil back in his helmet—for *pockets* had not been invented in those days. "And I dare say", he continued, "that you will now find that you have sufficient premisses to enable you to deduce the truth of Z."

At this point the narrator, having some pressing business at the Bank, was obliged to leave. He overheard Achilles say, however, something or other about the off-side rule and the advisability of 'not handling the ball unless you are the goalkeeper'. The Tortoise replied that philosophy was 'nowadays a much better game than football'. On that most charming note the narrator drove off, and left the happy pair.

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A SUPPOSED CONTRADICTION IN *LE CONTRAT SOCIAL*

In a recent Note in *MIND*, January, 1950, Mr. F. A. Taylor suggested that Rousseau was less concerned with asserting the infallibility of the general will than in stressing its moral nature: it is 'droite' in the sense that it seeks 'le bien commun'. His article serves as a useful introduction to the problem of Rousseau's contradictions, a problem which Mr. Taylor does not attempt to solve, contenting himself with quoting Professor Cole's words in the Introduction to the Everyman edition of *The Social Contract*. "It is impossible to acquit Rousseau in some of the passages in which he treats of the General Will of something worse than obscurity-positive contradiction." Alas, Professor Cole does not try to explain for us these contradictions: "... These difficulties the student must be left to worry out for himself".

Were a satisfactory and simple answer to the problems raised by the General Will to be produced, at least one favourite examination question would have to be reconsidered. All I propose to do is to see if it is possible to find out *why* in certain places Rousseau appears to contradict himself. If

- (a) we apply H. A. Prichard's distinction between what is 'morally good' and what is 'objectively right',
- (b) we remember the distinction between analytic and synthetic propositions,
- (c) we forget Hegel's use of words like 'personality',
- (d) we read carefully the words of Rousseau which follow his alarming pronouncements on counting votes as the way to ascertain the General Will,

then perhaps we may discover why Rousseau was led to say what he did.

For the sake of simplicity I shall deal with only one of the contradictions. In Book II, ch. 3, he writes:

Il s'ensuit de ce qui précède que la volonté générale est toujours droite et tend toujours à l'utilité publique; mais il n'ensuit pas que les délibérations du peuple aient toujours la même rectitude. On veut toujours son bien, mais on ne le voit pas toujours. Jamais on ne corrompt le peuple, mais souvent on le trompe; et c'est alors seulement qu'il paraît vouloir ce qui est mal.

In the same vein he writes in IV, i:

S'ensuit-il de là que la volonté générale soit anéantie ou corrompue? Non: elle est toujours constante, inaltérable et pure; mais elle est subordonnée à d'autres qui l'emportent sur elle.

On the other hand, in IV, ii, he states:

... et du calcul des voix se tire la déclaration de la volonté générale. Quand donc l'avis contraire au mien l'emporte, cela ne prouve autre chose sinon que je m'étais trompé, et que ce que j'estimais être la volonté générale ne l'était pas.

It is this quotation which is often torn from its context and will therefore be dealt with in connexion with (d) above. Leaving aside this issue for the time being, we have, roughly speaking, two propositions :

- i. The General Will is always right.
- ii. The General Will is found by counting votes.

How are these to be reconciled ?

(a) Even if we agree with Mr. Taylor that Rousseau was laying no claim to the infallibility of the General Will, the connexion between morality and vote counting seems obscure. It becomes plainer by the application of Prichard's method in "Duty and Ignorance of Fact". The example I shall use is my own, not Prichard's.

Let us assume that every Wednesday I visit my ageing mother at 2 p.m. This is an act both 'morally good' and 'objectively right'. On one Wednesday just before I leave home at 1.45, my wife, who is in the kitchen, is stabbed by an intruder. Unaware of the attack, I go off as usual to visit my mother. Have I done the right thing ? I certainly have not done the 'objectively right' act, but I can still be said to have done the 'morally good' one, owing to my ignorance of the circumstances. Rousseau would seem to have been aware of this difficulty when he wrote : "The people are never corrupted though often deceived". His suggestion that a Legislator is necessary to furnish a primitive society with wisdom is made with this in mind. The more people know about the circumstances, the more likely they are to conform with the General Will. Yet it is by no means certain that Rousseau was clearly aware of any ambiguity when he wrote : "The General Will is always right". It is probably best, however, to accept Mr. Taylor's suggestion that the English translation of 'droite' gives an 'objectively right' interpretation not present in the French.

But there is a further problem. Suppose I have just learnt that my mother has been taken seriously ill, and go to tell my wife in the kitchen. She has not been wounded, but the intruder has threatened her and she feels a little faint. To whom have I the greater obligation ? Some might say that my mother, others that my wife, needed my presence the more, even though we might all agree on the facts of the situation. There would thus be a disagreement on a moral issue, even though everyone would be advising what they thought to be right. It is this sort of disagreement which Rousseau never seems to have considered. However well-educated his citizens may be, and however civil their religion, is there any reason to suppose that they will always agree on moral questions ? The example quoted above deals with a particular issue, whereas the General Will is concerned with general issues, such as the relative importance of mutually exclusive obligations to wives and mothers, but these involve the same possibilities of moral disagreement. Even though Rousseau may have meant by 'right' 'morally good', it is clear from his remarks on minorities that he never

considered it possible for *both* sides in a dispute to be morally good.

(b) Another approach to the problem of Rousseau's contradiction in these propositions is by way of an inquiry as to whether the propositions are synthetic or analytic. Is the proposition: "The General Will is always right" similar to "A triangle has three sides", or is it of the type "The Victoria building of Liverpool University is red"? The former is analytic: the latter is synthetic, being an empirical statement verifiable by the gloomy experience of walking up Brownlow Hill. If the proposition "The General Will is always right" is analytic, then rightness is necessarily implied in the concept of the General Will. This is what Rousseau would appear to be maintaining, but he could hardly go on to say that "The General Will is found by counting votes" was a proposition of the same sort. For this is an empirical statement, which cannot be discussed in the same way as an analytic one. It must not be imagined that Rousseau thought they they could be, but it certainly does look as if he never recognised that there was this difference between them. It is doubtful whether he had read Leibniz, and of course he preceded Kant. Modern students who sense immediately that the statements are different in kind, may not realise wherein the difference lies.

(c) It is important to remember that Hegel, like Kant, wrote later than Rousseau. The author of the *Contrat Social* was not concerned with Ideas and Absolutes. Indeed, he introduces his book with the words:

Je veux chercher si, dans l'ordre civil, il peut y avoir quelque règle d'administration légitime et sûre, en prenant les hommes tels qu'ils sont, et les lois telles qu'elles peuvent être.

He was concerned with men as they were, and the Legislator was, at an early stage, to be a real man. The spirit of the laws which would later take his place was a tradition of political behaviour of which Burke would have approved. Admittedly Rousseau uses phrases like 'la personne publique' (III. i.), 'cet acte d'association produit un Corps moral et collectif' (I. 6.), but this reference to the state as a personality would seem to be metaphorical and not transcendental. In Book I, Rousseau is stressing that moral element of his philosophy which distinguishes it from that of earlier writers like Hobbes: he was not to know of the peculiar construction that readers of Hegel would put upon his references to a 'moral personality'. When he asks what a law is in Book II. ch. 6, he says:

Tant qu'on se contentera de n'attacher à ce mot que des idées métaphysiques, on continuera de raisonner sans s'entendre.

He was not trying to be metaphysical when he introduced his General Will and it is possible that reference to Idealist theories only obscures his meaning. After all, there is nothing mysterious about the British reference to the Crown. It is dangerous to apply

Hegel to Rousseau, for, though they occasionally use the same words, they mean vastly different things by them.

(d) Lastly, there must be considered the question of Rousseau's words that the General Will is found by counting votes. That he should state in Book IV. ch. 2. :

Plus le concert règne dans les assemblées, c'est-à-dire plus les avis approchent de l'unanimité, plus aussi la volonté générale est dominante

depends, as we have seen, on his assumption that there will be agreement on moral issues if people try hard enough. Even so, it is very hard to agree that

Quand donc l'avis contraire au mien l'emporte, cela ne prouve autre chose sinon que je m'étais trompé . . . (IV. 2.)

But closer reading shows that Rousseau covered himself by careful definition. He assumes throughout the paragraph that the voters are searching their consciences. If we argue as we often do, that majorities are not always right, we are forgetting that Rousseau's is no ordinary, perhaps accidental, majority. As if to emphasise that this is not the case, Rousseau states the real nature of the situation even more clearly in the next paragraph :

Ceci suppose, il est vrai, que tous les caractères de la volonté générale sont encore dans la pluralité.

It is this important passage which is neglected by commentators, and yet which is of supreme importance. For if we find the General Will by counting votes, thus getting the majority view ; and if the majority view expresses the General Will only if 'all the qualities of the General Will still reside in the majority'—then the argument is nothing more than a tautology.

Such, therefore, is the conclusion we are led to in our study of an apparent contradiction in Rousseau. Unaware of the ambiguities in the word 'right', he let himself be morally intoxicated by the inspiration of an 'infallible' General Will. It was not an excuse for preaching a gospel of self-righteousness, otherwise he would not have used illustrations where he says : If I am in the dissentient minority, I must admit I am wrong. He asks the question : How can two men who differ both be right, and he does not see that they may both be wrong (objectively) and yet both be morally good. Not only ethical but also logical distinctions seem to elude Rousseau, and as he is bent on finding a principle of action which is both pure and practical, he cannot avoid using both analytic and synthetic propositions, again unaware of the complications that ensue. On the other hand, it is making Rousseau unnecessarily obscure to introduce metaphysics into the analysis : the General Will has nothing in common with Plato's Forms or Hegel's Absolute.

Finally, he was not suggesting that the majority is always right. His failure to detect an ambiguity in the word 'right' led him to the position where, if his General Will was to be the practical thing he

hoped it would be, some class of decisions could be labelled 'right'. He got perilously near to asserting that the majority was right, but common sense, presumably, stopped him. It made him introduce the presupposition that the General Will resides in the majority. The fact that he thus became tautologous may indicate that he reached an impasse: it does not permit us the facile inference that in Book IV he changes his argument and identifies the General Will with the majority. It is dangerous to underestimate or overestimate a writer's ability: yet strangely enough with Rousseau we do both.

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REFERRING USES AND SELF-ENFORCING DIRECTIVES

"WHAT is referring?" "If language is about the world, what is aboutness?" "What is semantic reference?" Modern philosophers are usually too wary to be ensnared by these invitations to nonsense, and we would be most surprised if anyone to-day began to answer saying "Referring is . . ." or "Aboutness is a relation which . . .". Nevertheless there are things which may legitimately be said when such a question is asked. In an article entitled "On Referring" (MIND, July, 1950), Mr. Strawson has said a great many of these things. He has not specifically posed the question "What is referring?", but by exhibiting and classifying the different ways in which some referring expressions may be used, by showing how different kinds of conventions serve to co-ordinate referring expressions and their referents, he has illustrated the legitimate ways in which this logically suspect question might be tackled. In this note I wish to call attention to one curious feature which, so far as I can see, all referring expressions, and only referring expressions, have in common. I hope thereby to give another illustration of the sort of thing which may reasonably be said in answer to questions like "What is referring?"

Take any expression 'e' and form an apparent order, directive, or request of the form "Turn your attention to e", "Please consider e", "Think about e". Now where 'e' is not a referring expression the result will generally be nonsense, or at best difficult to understand and interpret correctly. Thus in the clearly nonsense category:

- (a) Think about is blue.
- (b) Turn your attention to separates the wheat from the chaff.
- (c) Please consider to the left of.

Here the inserted expressions cry out for the protection of inverted commas, and without them these directives and requests are sheerest nonsense. In the at best dubious class we find:

- (a¹) Think about blueness.
- (b¹) Turn your attention to transitivity.
- (c¹) Please consider brotherhood.

Even here we may well be in doubt as to correct interpretation. We are likely to feel that these too are best handled with the aid of inverted commas or some other device, that, *e.g.*, the most useful way to take (c¹) would call for a reformulation as:

- (c¹)₁ Please consider ' - - - is a brother of . . . '.
- or (c¹)₂ Please consider *the relation of brotherhood*.

I have no desire to enter into controversy as to whether by inserting an expression in inverted commas one forms the *name* of that expression, or, for that matter, of anything else. I would merely call attention to the obvious fact that by writing an ex-

pression and then guarding it with inverted commas we normally indicate that we are talking about (*i.e.*, *referring to*) that expression, rather than using it. Applying this result to our previous examples we note that when the variable in our rubrics for directives and requests is replaced by a referring expression the result is a form of words which seems to be readily intelligible, and which we apparently know how to obey or accede to.

There is, however, a systematically curious feature of such directives and requests, when 'e' is replaced by a referring expression. It is a feature which seems not only to call attention to a very general characteristic of referring expressions, but also to mark off such directives and requests as so unusual that we may indeed hesitate to call them 'directives' and 'requests' at all. Let us take some more examples where inverted commas would be clearly unnecessary.

- (1) Turn your attention to the British monarchy.
- (2) Think about Caesar.
- (3) Please consider the Eiffel tower.

The systematically curious feature of this kind of directive is that, in the most usual sense of 'disobey' or 'fail to comply', we cannot (*logically cannot*) disobey or fail to comply with these orders and requests. We can, of course, fail to understand them. And if one can be said to have *disobeyed* an order, when in fact he has not even understood it, then in this sense these can be disobeyed. But if, as is surely most often the case, disobeying an order involves understanding it *and* failing to comply with it, then these "orders" cannot be disobeyed. The explanation (which justifies the logical 'cannot') is obvious. Doing what these orders and requests ask of us just is understanding the referring expressions which they contain. I do not wish to debate the terminological question as to whether this peculiarity of these orders and requests is such that they should not be called 'orders' and 'requests'. What I do want to insist on is that, whatever we call them, they are quite intelligible and fully meaningful. It is not that we are in doubt as to what to do when we hear one of them, it is rather that we find that we have already done it.

At this point one can almost hear the practitioners of a latter-day "contextual criticism" sharpening their knives. What is curious they suggest, is not these directives and requests, but rather the state of mind of anyone who would consider them thus out of *context*. If I would but examine them in the sorts of circumstances in which they are actually used, their queerness would disappear. To this objection there are two replies. First, I shall subsequently draw attention to one sort of context in which such directives and requests are actually used, and in which they perform just such an attenuated function as my consideration of them would lead us to expect. Secondly, it must be admitted that there is nevertheless a certain artificiality in my consideration of these directives. But this, I would maintain, is an inevitable corollary of my attempt to say

something about referring expressions as such. In actual language we refer for many different purposes, just as we have many different methods of referring. But it is at least doubtful if we ever refer for no purpose at all, if referring for its own sake is a recognized linguistic phenomenon. Yet in asking a general question about referring itself, I am deliberately ignoring the different kinds of purposes for which we refer. The queerness of these directives consists exactly in this: When one of them has been issued and understood both speaker and hearer have succeeded in referring, but no progress has been made. A reference has been established, but it has not, as yet, been used. It is, of course, for this very reason that I think these directives help to throw light on referring as such. If the contextualists point out that my queer results serve to exhibit the queerness involved in an investigation of referring as such—this is unobjectionable. 'Queer' in 'This is a *queer* question' is now being used as a synonym of 'philosophical' or 'non-factual' or the like; and I will cheerfully plead guilty to conducting a philosophical or non-factual investigation.

It will have been noted that in my examples I have consistently preferred the rubric 'Turn your attention to . . .', as against the perhaps more common 'Attend to . . .', or 'Pay attention to . . .'. I have done this because the latter expressions are at least equally susceptible to interpretation in the sense of 'Fix your attention on . . .', and I do not think that this last rubric shares the curious feature of my examples. It seems to me that I might both understand a request to fix my attention on the British monarchy, *and* deliberately refuse to comply with it. In any event the rubrics 'Turn your attention to . . .' and 'Please consider . . .' do have at least one clearly accepted use. In that sort of writing or speaking which is called 'connected discourse' they occur at transition points, and the requests or directives thus generated have a merely rhetorical force. Imagine an essay on totalitarianism. The author has just finished a section on Nazism in Germany and he leaves an extra wide gap and begins a new paragraph:

(I) Let us now turn to consider Italian Fascism. It had its origins in . . .

If one had to characterize the first sentence of this new paragraph he might well say that it had a merely rhetorical force, or that it served a merely stylistic purpose. If we understand it at all it has already done its job of directing our attention to what will be the subject-matter of the next bit of the essay. The author could equally well have put 'Italian Fascism' as a chapter or section heading in the space above the new paragraph, and then begun instead:

(II) Italian Fascism had its origins in . . .

The important point is that, in some curious sense of 'equivalence', (I) and (II) are equivalent; and this is exactly what our investigation of these self-enforcing directives would lead us to expect.

I have suggested that by employing these rubrics and replacing the variables by various different expressions we can distinguish referring expressions as those whose insertion gives us a readily intelligible, but nevertheless curious, directive or request—one with which non-compliance is impossible. As an illustration of the applicability of this technique I shall consider the philosophically sanctified gesture of pointing, and by implication its constant companion "ostensive definition". In their puzzlement about our very question—"How does language refer?"—some philosophers have certainly written as though they considered the gesture of pointing some sort of *natural* mode of reference. But the application of our technique to either 'Bring your attention to this', (accompanied by the pointing gesture), or to the gesture itself interpreted as a directive or request to attend to something, yields our now familiar curiosity—the self-enforcing directive. The gesture, like the chapter heading, accomplishes its purpose if we but understand it. 'Bring your attention to this', accompanied by the gesture, is like the merely rhetorical sentence in (I).

Our result serves to assimilate this gesture to the general class of referring expressions, and to this extent to eliminate some of its mysteriousness. It may be that the aura of philosophical uniqueness attaching to this gesture can be further undermined by suggesting that its alleged naturalness as a means of referring is in fact a misinterpretation of this curious impossibility of non-compliance with the request or directive which it embodies. This suggestion is strengthened by reflecting that if one had to choose between (i) turning one's attention to, and (ii) fixing one's attention on, as descriptions of what pointing accomplishes, (i) would be more plausible than (ii). If, through a dim recognition of this impossibility of non-compliance, philosophers have sometimes accorded an unduly privileged position to the pointing gesture, then two comments are in order. First it must be emphasized that this is a *logical* impossibility, and that it applies only *if* the gesture is understood. Secondly it must be noted that this same impossibility, under the same conditions, extends to those directives or requests whose references are as verbal as you please. A full acceptance of these two points should lead us, as philosophers, to be extremely sceptical of pseudo-psychological "bow-and-arrow" theories of meaning.

Nothing that I have said should be taken as denying that there are important differences between:

- (4) That is the tallest man-made structure in Europe. (Uttered at the Trocadero in Paris and accompanied by a gesture pointing across the Pont d'Iena.)
on the one hand, and
- (5) The Eiffel tower is the tallest man-made structure in Europe.
on the other. These differences are to be found in the differing conventions governing the referring portions of (4) and (5).

Specifically, the logical structure of (4) is to be laid bare through an analysis of those referring conventions which govern token-reflexive uses. For both the word 'that', and the gesture of (4) are characteristically token-reflexive.

But this analysis cannot even begin until it is clearly recognized that the pointing gesture, with or without an accompanying demonstrative, is after all a referring "expression". As such it is as thoroughly convention-governed as 'the Eiffel tower' or 'the tallest man in London', and indeed its conventions are a good deal more systematic, in Strawson's terms, less *ad hoc*, than those which govern the use of 'the Eiffel tower'. In the sense of 'natural' in which natural signs are sometimes opposed to conventional signs, the pointing gesture is certainly not *natural*.

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THE IMMORTALITY OF THE SOUL

MR. A. H. BASSON in his "Dialogue" in the January number of *MIND*, 1950, says: "to speak truly of the destruction of Plato's soul, it must be impossible for you to conceive of the existence of anything exactly resembling it" (p. 33).

(1) Supposing that the term, "Plato's soul" is free from ambiguity, there are other grave difficulties to the argument. It is assumed that (a) Plato's soul will be re-born in a *human* body; (b) that "Survival" entails "re-birth"; (b) is true if, and only if, it is true that the soul is *never without* a body. Even if this is true, it does not follow that "re-birth" (of Plato's soul) will be in a *human* body. "Being re-born" means being re-born in *some* body, human or non-human. This, which I shall call The Principle of Uncertainty, must be admitted, though I dislike it.

(2) "Re-birth" entails "survival" (not that survival entails re-birth), but it is logically possible for a soul to survive the death of its body and yet not to be "re-born" at all. There are two things to be distinguished: (a) the fact that it is re-born at all, (b) the fact that it is re-born in a human or non-human body. Both these must be governed by the same hypothesis, if there is any.

(3) Is there a hypothesis which governs both (a) and (b)? In Hindu and Buddhist thought, there is, in fact, a hypothesis; this is called *The Law of Karma*. The main question is: what is its logical character? In the same number of *MIND*, Professor C. D. Broad, in reviewing Kneale's book on *Probability and Induction*, draws our attention to what Kneale calls "explanation by means of *Transcendent Hypothesis*". Its peculiarity is stated by Broad thus: "The peculiarity of a transcendent Hypothesis is that the things and processes in terms of which it is formulated *could not* conceivably be perceived by the senses, and therefore, strictly speaking, could not be imagined either" (p. 98). I would say that the Law of Karma offers the *sort* of explanation, of certain circumstances, known as "explanation by Transcendent Hypothesis". It is possible that the Law of Karma is *based on* some other Law or Laws based on good inductive evidence, but *in the nature of the case*, the Law of Karma *could never* be established by direct induction. "To state a Law properly we need a conditional sentence", it is said by Kneale; the Law of Karma is *conditional* and, the *condition* it expresses is: *Yathā Karma, Yathā gratham*: "according to their deeds, according to their knowledge", (Hume's Tr.: *The Thirteen Principle Upanishads: Katha Upanishad*, 5. 7); i.e. according to its deeds and according to its knowledge will "Plato's soul" be born in human or non-human body if it is re-born *at all*.

In the same number of *MIND*, Einstein is quoted by Max Born to say: "you believe in a dice-playing god, and I in the perfect rule

of law in a world of something objectively existing which I try to catch in a wildly speculative way" (p. 124). The Law of Karma is an attempt "to catch in wildly speculative way" the rule of Law and, to save ethics and Religion from a "dice-playing god".

(4) But the truly important problem of Immortality remains. Does Immortality mean "survival" and "re-birth" (the series of re-births) under the inevitability of the Law of Karma?; or, does it mean "survival" and *freedom from* re-births? Mr. Basson says: "we should expect, and with good reason, that when we ourselves came to die, we should afterwards find ourselves in other circumstances. The thought of death would lose its power over us" (p. 32). Yes; but does not the fear of the *Unknown* still face us under the form of "re-birth" and its "in other circumstances"?

If we would state the problem of Immortality, we shall have to restate the problem of "re-birth". We must distinguish between: (a) the circumstances under which a soul (e.g. Plato's soul) is compelled to take birth in *some* body under the inevitability of the Law of Karma: and (b) the circumstances in which there is "re-birth" (or *birth*) without the compelling Law of Karma. It is the latter possibility that throws more light on the problem of Immortality. Is (b) possible? It may be possible: this is the meaning of *avatar*, a "descent", of spirit into matter, of the divine into the human. To use Plato's words in the Allegory of the Cave, "having ascended and arrived at the good", there are some who, in their goodness and freedom, "descend" into the limitations of finite life to bear the cross and to "save" the world. Or, if they are also ruled by Necessity and are "to be made to descend again among the prisoners in the den", to quote Plato, the sort of Necessity which compels them to return is utterly distinct from that kind of Necessity by which ("they" i.e. transmigrating souls under the Law of Karma), "are driven all manner of ways like shooting stars to their birth", as Plato puts it, in the Vision of Er, in the *Republic*.

(5) It will be noted that in my argument I have distinguished between "survival" and "re-birth" and I have assumed them to be logically independent but connected by the Law of Karma. I have also distinguished between "being born" in two senses: "being born under the Law of Karma" and "being born" *independently* of it. My method has been to assert a condition and then to negate it. In my notion of "survival" there still lurks the idea that Immortality is something which is to be experienced *after* the death of the body. I must negate this condition also. Immortality must mean an *experience* here and now, in this body *prior* to death: *mṛtyu pāśān purataḥ pranodya*: "having cast off in advance the bonds of death" (Hume's Tr. *Katha*. Upanishad, 1. 18). Then, and only then, to quote Mr. Basson, "the thought of death loses its power over us".

N. A. NIKAM.

A NOTE ON "MEETING"

MR. MACPHERSON has made a number of valuable criticisms of the suggestions which I put forward in my article "The Significance of Christianity" (MIND, April, 1950): unfortunately he has included among those criticisms the assertion that what I want to do cannot be done. I want to repeat my reasons for thinking that it can.

Before I do that I should mention that what Mr. MacPherson calls my "disarmingness" was not intended to disarm criticism, but to evoke it. It was a genuine admission that my article stands at the beginning of a journey, and not at the end. It was not intended as a first step, but as a signpost to direct that step. I must accept Mr. McPherson's assumption that in one respect it involved a step, and I must justify the taking of that step. This is, of course, my suggestion that "meeting God" can be offered as a phrase of which the meaning can be understood.

I am not going to argue that events which I should call "instances of meeting God" have occurred, or that they can occur: I am not going to indicate what I think such an event would be. My intention in this note is to show that there are events which may be appropriately described as "instances of meeting a person" which do not involve the sense-data which are associated with the presence of a human being.

I do not accept Mr. McPherson's contention that it is wrong to use the word "meeting" of a telephone conversation: certainly to say that two armies "meet" in battle need not mean that any individual soldier on either side sees or hears any soldier on the other. The "meeting" may well be mediated by artillery, firing from behind the lines.

If Mr. McPherson insists on using the word "meet" in his restricted sense, then I am ready to consider an alternative word to describe such relationships as a telephone conversation, the first stages of a battle, a discussion such as that in which Mr. McPherson and I are engaged, a game of chess by letter, or a tug of war in which the rope passes round a corner. Such relationships are personal relationships, and if "meet" will not do then we must find another word. Mr. McPherson implies that no other word can be found—if he is right, then so much the worse for the English language: the events occur, and they are instances of a definite kind of relationship, and that is all I need for my argument. I shall describe them as "meetings".

The events to which I refer are characterised as follows :

- 1 They are relationships between persons.
2. They involve sense-data which mediate the relationship.
3. They do not involve the sense-data which we associate with the presence of a human being.

The first two marks justify the use of "meeting"; the third distinguishes this sort of meeting from other kinds of meeting.

It is clear to an outside observer that these events are relationships between persons. I have to show that when I meet someone in this way I may have reason to believe that I am meeting a person—that is, that there is a specific difference, for instance, between pulling on a rope which passes round a corner and is fixed to a strong spring, and pulling on a rope which is being pulled by someone else who is round the corner. It is not necessary to show that I shall always judge rightly: the fact that we may at first think that we have met a person when we walk towards a waxwork does not alter the fact that there is a specific difference between meeting a person and meeting a waxwork.

Mr. McPherson himself has pointed out that we should hardly speak of "a meeting" if two people passed in the street without making any sign to show that they were aware of each other. When we meet a person directly not only are the sense-data connected with the human-being involved, but changes in these sense-data take place. We call such changes "scowls" and "smiles" and the like. There are also, sometimes, changes in more intimate data which we call "having our hand shaken", and "being hit" and so on. These things are signs, and it is because we can relate these signs to the character and intentions of a person that we speak of the relationship as "personal". In effect these signs enable us to make judgments about the person whom we meet.

The "meetings" which I have instanced are also characterised by changes in patterns of sense-data (internal as well as external), and these changes are, again, signs. These signs can lead us to judge that we are meeting (indirectly) a person: if we make this judgment we can then go on *exactly* as before. It is, of course, true that in the common McPherson meeting there is a much stronger *prima facie* case for believing that we are meeting a person, but I think that most of us would claim that if we were holding a rope which passed round a corner it would not take us long to decide, simply by pulling, whether there was a person on the other end or not.

My case is that there are events which are "meetings" with a person which do not involve the seen or heard presence of that person: that we may have reason to believe very strongly that we have "met" a person although we never see or hear him: that we are able to form judgments about the character and intentions of that person although we never see or hear him: and that all these

judgments are, in principle, as valid as the corresponding judgments which we make when we meet a person directly.

I said that "meeting God" was "not usually marked by the sense-experiences which we *commonly* associate with meeting a person": I was careful not to say that it would "not be marked by sense-data". I should prefer to have said that it was not "characterised by the sense-data which we commonly associate with the presence of a human-being", but the advantage of this statement is only greater clarity: it does not indicate any change of thought. I should, perhaps, add that no "religious experience" occurs in the absence of all sense-data.

DAVID COX.

NOTE ON THE EXHIBITION OF LOGICAL MACHINES AT THE JOINT SESSION, JULY 1950

AN exhibition of logical machines arranged from Manchester University was held at the Joint Session of the Aristotelian Society and Mind Association at Bristol, 7-10 July, 1950. Two electrical relay machines were shown—a three-variable machine built at Ferranti's¹ and a multi-variable one now in process of construction at Manchester. Two small computers for transforming groups of binary digits (representing combinations of propositions) into terms of logical symbolism were also exhibited; groups of digits of up to 2048 places may be dealt with.

The first logical machine was constructed by Stanley Jevons at Manchester and was largely operated by pulleys and levers (cf. *Philosophical Transactions Roy. Soc.*, 1870). Jevons tells us that he often used it in his class lectures when he was Professor of Logic at Owens' College (now the University of Manchester) so as to give a "clear and visible analysis of logical problems of any degree of complexity". With the application of Boolean algebra to electrical switching circuits, it is now possible to construct electrical machines which can carry out operations in Symbolic Logic involving relations between a number of variables. The logical operations of conjunction and disjunction can be represented by series and parallel connexions of relay circuits.²

A logical relay machine working by means of truth-tables was built by Kalin and Burkhart at Harvard in 1947. This machine appears at the most to be an electro-mechanical version of the Jevons' machine, and though it is a multi-variable machine it does not seem to be able to perform mediate inferences directly. It only gives the set of conclusions (or immediate inferences) compatible with the initial premises.

The Ferranti machine exhibited at the Joint Session deals with the logical relations between three propositions or variables A.B.C. These variables and their functions are represented in the form of binary numbers stored in electro-magnetic relays, "On" (1) standing for truth, "Off" (0) for falsehood. The state of each relay is indicated by a lamp. The initial propositions are set up in the form of truth-tables, and synthesised together by means of the logical operators \cdot , \vee , \equiv , \supset , \sim . The resultant truth-table is then transferred and stored in another part of the machine for later use, whilst fresh formulae are operated on. The stored

¹ Cf. *Nature*, vol. 165, p. 197, 4th Feb., 1950.

² Cf. "A Symbolic Analysis of Relay and Switching Circuits", Claud E. Shannon, *A.I.E.E. Transactions*, vol. 57, 1938, December section, pp. 713-723.

propositions can then be returned to the operational part of the machine and together with other formulae have further logical operations performed on them. This process can be continued so as to deal with expressions of any degree of complexity containing three variables. Mediate inference is performed by the elimination of variables (middle terms) by a process analogous to the projection of a three-dimensional pattern on a two-dimensional plane. This analogy is precise if one thinks of truth-tables dimensionally, each variable representing a dimension; mediate inference on this view consists in projecting from an n dimensional truth-table to one of $n - 1$ dimensions.

The second machine is based on similar theoretical principles but operates on a serial scanning system and is independent of the number of variables involved. After being set to the formula under investigation it performs simultaneously all operations necessary to give the relationship between a pair of the original variables or two functions of the original variables (an elimination operation). The time taken to complete the problem is thus dependent on the number of independent variables in the original formula. Thus a three-variable problem takes approximately $\frac{1}{15}$ second, and a twenty-variable problem would take 3 hours. The machine consists of a number of separate units which are connected together in accordance with the formula being investigated. Any number of eliminations may be performed simultaneously. The whole machine can be extended unit by unit.

Jevons' machine worked on the method of indirect demonstration or inference. This consists in forming a Boolean expansion of the terms in the premises, *i.e.* forming all their possible combinations and then striking out the combinations which are inconsistent with the premises. Those remaining form the conclusion. The method of indirect inference is equivalent to Euclid's indirect demonstration or *reductio ad absurdum*, *i.e.* by showing that two out of three possibilities lead to a contradiction one establishes the truth of the third. This is a tedious method but it can be considerably shortened as Jevons showed by the use of mechanical devices.

It is interesting to note that truth-tables for two and three terms were put forward by Jevons as far back as 1870. The truth-table for three terms called the logical index contained 256 entries.¹ Indeed Jevons' machine worked by a mechanical truth-table, a pin attached to a lever standing for the negation of a term, absence for its appearance. The pattern of pins is precisely that of a truth-table except it is in reverse. When a combination of keys representing specific terms are pressed their negatives disappear from the indicating panel, leaving behind the positive forms of combinations which give the answer to the problem.

¹ Cf. *Studies in Deductive Logic* (Macmillan, 2nd edition, 1884), pp. 286-289. What in effect is a truth-table for two terms is given in *Principles of Science* (Macmillan, 2nd edition, 1877), p. 135.

The machines exhibited at Bristol operate by means of truth-tables. They synthesise propositions, atomic or molecular, by the direct application of simple logical rules. The rows of binary digits representing the propositions are synthesised to give appropriate truth-tables.

To give examples, where p and q are two propositions, and the truth values T and F are represented by the binary numbers 1 and 0.

$p. 0101$	$p. 0101$	$p. 0101$	$p. 0101$	(x)
$q. 0011$	$q. 0011$	$q. 0011$	$q. 0011$	(y)
$p.q. 0001$	$p \vee q. 0111$	$p \equiv q. 1001$	$p \supset q. 1011$	(z)

It will be seen by inspection that if x and y are digits in the binary representation of p and q , and z the digit in the resultant, the rule for

conjunction is $z = 1$ when x and y are 1, otherwise $z = 0$
disjunction is $z = 1$ when either x or y or both are 1
equivalence is $z = 1$ when $x = y$; $z = 0$ when $x \neq y$
implication is $z = 1$ when $x = y$; or when z is 0 and y is 1.

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VIII.—NEW BOOKS

Fundamentals of Symbolic Logic. By ALICE AMBROSE and MORRIS LAZEROWITZ. Rinehart and Co. Inc., 1948. \$5.00.

THIS book is intended for the beginner in formal logic. The first half of it consists mainly of a comprehensive account of the system of truth-functions; in the second half, the basic notions of the lower functional calculus and the calculus of classes are introduced, and their relations to the forms and rules of Aristotelian logic are discussed at length. The main, and considerable, merit of the book is that the strictly technical material is presented simply and clearly. Where the authors have been less successful is in their account of the relations between the formal systems they describe and the logic of ordinary speech. The way the logician's symbols are related to one another stands out clearly; the way they are related to the words we daily use is obscured. There is a pedagogic temptation to induce learners to accept formal logic as 'natural', and of quotidian relevance, by suggesting that those symbols are just some of these daily words in logical undress—stripped for logical action; with the hint that the initial strangeness of the identification will soon pass off. And so, unfortunately, it does.

In the first two chapters, the notions of form, necessity, propositions, of the variable and the propositional function (by which the authors understand, roughly, the meaning of any propositional formula containing one or more free variables) are briefly expounded. The formal logician is said to be interested only in those cases of inconsistency, validity, etc. in which the logical relation in question holds independently of the specific subject-matter being discussed. A list of formal concepts, acknowledged to be incomplete, is given; a formal function is defined as a function containing, in addition to variables, none but formal concepts; and the form of a proposition is defined as the formal function it exemplifies. This account of form is infected by an inconsistency in the account of propositions. A proposition is defined as "the literal meaning of an indicative sentence" (1). It is also spoken of as either true or false (2). There are, however, countless indicative sentences which are unambiguous, i.e. which express *one* proposition (1); but which are, on different occasions, used to make different statements, some true and some false, i.e. which express many different propositions (2). Otherwise than by giving odd meanings to 'sentence' or 'ambiguous', the inconsistencies can be avoided only by dropping (1) or (2). Since the usage according to which a proposition can be either true or false, but not both, is a reputable one, it seems preferable to drop the definition. The authors' section on logical necessity is short; and it might have been preferable to connect it more closely with the topics of logical relations such as entailment or 'superimplication' (pp. 84-89) and of validity (pp. 120-121).

The exposition of the system of truth-functions occupies chapters III-VIII. Topics treated are, in order: the interdefinability of the constants of the system; generalised propositional analogues of the De Morgan theorems for classes; punctuation- and formation-rules for truth-function expressions; the stroke-function; Boolean expansions; the truth-table method of exhibiting logical relations and of establishing

whether truth-functions are tautologous, inconsistent or contingent; a second testing-method, employing Boolean expansions; and finally, in a well-done chapter, the calculus, or deductive system, of truth-functions. Two minor blemishes are: a slightly confused discussion on page 81, where the phrasing is such as to give the impression that any conjunction of disjunctions is inconsistent unless each of the disjoined functions of either conjunct is compatible with each of the disjoined functions of the other conjunct; and the repeated assertion that the deductive system "generates" the tautologous subclass of truth-functions—an unnecessarily mystifying way of pointing the contrast between a deductive, and a testing, method.

The main defect of this part of the book is that which I have already mentioned. After indicating that sentences employing such conjunctions as 'if', 'and', 'or' are to be regarded as expressing "compound propositions", the authors state roundly: "Formal functions which are also the forms of compound propositions belong to that special class of functions called 'truth-functions'" (p. 24). On pages 30-34, it is acknowledged that there are some differences between the ordinary employment of the mentioned conjunctions and the meanings given to the truth-functional constants; but, apart from a reference to the exclusive use of 'or', this difference is dismissed as a vague requirement of 'relevance' or 'connection' between the propositions joined by the ordinary conjunctions. No wonder the authors later find themselves writing of the "strangeness" (pp. 46, 75) of the definition ' \supset '; and saying of such theorems as ' $\sim p \supset p \supset q$ ' that "they seem to be false and at the same time are provable rigorously" (p. 76)! Committed to their identification of truth-functional constants and ordinary conjunctions, the authors say, on page 91, of the two propositions *If it rains the crops will be good* and *If it rains the crops will not be good*: "One needs merely to note that both propositions are true under the condition that it does not rain, to see that they are consistent with each other"; and throughout chapter VII, they speak of certain tautologous functions as exemplifying "forms of argumentation of frequent occurrence" in ordinary speech. All this seems productive of confusions which the authors could quite easily have avoided by applying, not only to the formation-rules of the language of truth-functions, their own dictum that "an artificial . . . language can be made to meet certain specialised demands" (p. 60).

Similar criticisms apply to the discussion of general propositions in chapter IX. Predicative functions and quantifiers are introduced, and the ensuing discussion largely takes the form of a claim to give the classical propositional forms an "unambiguous" interpretation and to amend the classical rules of the Square of Opposition in such a way as to free them from inconsistency. The authors recommend that $\sim(\exists x).fx \sim gx$ should be accepted as an analysis of the A form of traditional logic and (in conjunction with $(\exists x).fx$, where existence of members of the subject-class is implied) of 'all'-statements of ordinary speech. As an example of the kind of argument used, I quote the following; "If A and E were so interpreted as to imply I and O, there would be no way of representing the forms of *vacuously* true general propositions such as 'All Greek gods have human frailties', that is, instances of $(x).fx \supset gx$ which are made true by the circumstances that $\sim(\exists x).fx$ " (p. 189).¹ It seems to me that vacuously true 'all'-statements (in this sense of 'vacuously true')

¹ My italics.

never occur in ordinary speech; and that the examples of propositions adduced by the authors (on p. 184) in support of their recommendations are either (1) propositions which would not be "held to be true" at all (but, rather, such that the question of their truth or falsity did not arise), or (2) propositions which would not normally be expressed by the use of 'all', or (3) propositions such as the "Greek gods" example quoted above, which is certainly not made true by the circumstance that there are no Greek gods. Nor does the explicit addition of an existential condition to the analysis save it from misrepresenting the function of 'all'; for the fulfilment of the existential condition is commonly a condition not merely of the truth, but of the truth-or-falsity, of what is asserted by the use of 'all'-sentences.

The author's claim to show that the traditional laws of the Square of Opposition are inconsistent is unfounded. What they show is that if the classical forms are given the interpretation they suggest, the rules are inconsistent. But if we are to start the process of interpreting the A, E, I and O forms as negatively and/or positively existential, we might as well continue it until we secure the consistency of the rules; which is not difficult. An alternative is to recognise that existence-propositions stand outside a logic of subjects and predicates altogether. They are presupposed by it; they do not belong to it.

There is an inconsistency in the authors' treatment of 'some'. On page 176, they say that 'Some foxes are sly' *usually means* that more than one fox is sly, the falsity of which does not guarantee the truth of 'No foxes are sly'. On pages 180 and 185, they say that in *ordinary English* propositions of the E and I forms are contradictory.

The remainder of this chapter is taken up with a brief account of functions of more than one argument, and of multiple generality; and an exposition of Russell's theory of descriptions.

At the beginning of chapter X class terms are introduced and the connexions between class and functional notation are explained. Some of the remarks about classes and functions are careless: e.g. "the members of the class are those values of the defining function which do not give rise to a false proposition when they are substituted for the variable" (p. 206). The remainder of this chapter and the whole of the next are devoted to an exposition of the analogues in the logic of classes of the traditional doctrines of immediate and mediate inference. Three methods are given of testing the validity of class-syllogisms: (1) the traditional 'rules', adapted to suit the class-interpretation; (2) Venn diagrams; (3) the antilogism method. The traditional doctrine of distribution of terms is given more serious attention than, perhaps, it deserves. In the final chapter of the book, a set of postulates for a calculus of classes is formulated and some theorems derived.

From time to time, the authors have an unfortunate tendency to write as if symbolic logic were full of surprises: some welcome ones, like the 'discovery' that the notation $fx \supset gx$, which can be used for singular propositions can also be made use of to express the form of generalisations (p. 167); some rather distressing ones, which nevertheless logic forces us to accept, like the 'paradoxes of implication' referred to above, and the existence of a null-class (p. 216). This tendency seems to be a direct result of the authors' evident reluctance to dispel the illusion that formal systems embody the logic of ordinary speech and writing, and to put in its place an accurate description of the relations between them.

At the end of each chapter is a set of questions and exercises.

I have noticed the following errors: on page 109, l. 26, for the first ' $\sim p \cdot q$ ' read ' $\sim p \cdot \sim q$ '; page 124, l. 20, for 'for' read 'against'; page 201, l. 23, for ' $(\exists x)$ ' read ' $(\exists y)$ '; page 201, l. 26, for the second ' fx ' read ' fy '; page 202, l. 2, for the second ' fx ' read ' fy '; page 241, l. 27, for " $ab = o$ " read " $ab = o$ ".

P. F. STRAWSON.

Zu Deutschlands Schicksalswende. By JULIUS EBBINGHAUS. Vittorio Klostermann, Frankfurt am Main. Pp. 164.

THIS collection of essays and addresses by Professor Ebbinghaus is of great interest for its account of the moral and political problems which had to be faced by Germans as a result of defeat in war and the collapse of the Nazi regime. Some of the articles are semi-popular in character—talks on the wireless or addresses which he gave to students in his capacity as Rector of Marburg University—while others are of a more technical kind. He deals with such topics as the function of the press and of the academic teacher, the duty of man under a tyranny, the responsibility of the individual German for the Hitler regime and for the war on humanity; and more generally with the collapse of tradition, the loss of principles, and the disturbed mind of German youth. His close reasoning is marked by a lucidity of exposition which rises at times to eloquence; and his general attitude is courageous, dignified, and just. If this were the authentic voice of Germany, there would be good hope for the future.

Professor Ebbinghaus, even in his most occasional utterances, draws his inspiration from Kant's moral philosophy—especially from Kant's philosophy of law, which is so seldom studied in this country. Apart altogether from its topical interest, this work has philosophic importance as an attempt to apply and develop the Kantian conception of 'Right'. This becomes most evident in the more technical essays such as the criticism of Marxism in *Sozialismus der Wohlfahrt und Sozialismus des Rechtes*. This criticism may be too *a priori* to suit English taste, but in spite of its difficulty it is by no means irrelevant to the present situation even in Britain itself. Professor Ebbinghaus combines strong personal convictions with an exceptional mastery of Kant's complicated technique, as is shown most markedly in his *Deutung und Missdeutung der Kantischen Imperatives*, an essay unfortunately not included in this volume and apparently no longer available in print. It is greatly to be desired that he should give as a systematic exposition of Kant's Critical doctrines, or at least of his moral and political philosophy. His present work, limited as it is, should be of interest to all students of Kant, and indeed to all students of Germany.

H. J. PATON.

Law and the Modern Mind. By JEROME FRANK. Stevens and Sons Ltd. £1 5s.

THIS book, a violent criticism of the conventional theory that what judges do is to apply to facts impartially ascertained a pre-existing and certain set of legal rules, was first published in the United States in 1930 when the author was still an advocate. Since then the book has run into five editions and the author has had a long experience as a judge which, as he claims in the preface written for the present sixth edition (the first to be published in England), has confirmed his main contentions.

The book is by turns shrewd and naive, acute and badly argued, stimulating and exasperating; it teems with references to philosophers (especially Vaihinger), psychologists and scientists, but it will be of interest to philosophers not for these but because it will bring to their notice many important discussions of the type of reasoning involved in legal decisions and statements of law which are to be found in the immense and exuberant flood of American legal writing.

The author's thesis is that most judges, lawyers and legal theorists have subscribed to "the basic myth" that the law which the judge administers consists of rules covering all the contingencies with which he has to deal and is something which he discovers and does not make. In the extreme form of the theory ("legal Platonism") the law is something never made by any man but as the theory's greatest critic, Oliver Wendell Holmes, termed it "a brooding omnipresence in the sky"; in milder versions the law is man-made, either anonymously in the remote past or by the fiat of a legislature. What is common to all versions of the myth is the view that the judicial function is simply to "apply" the law to "the facts of the case" by making rigorous logical deductions from independently discoverable legal rules. But "the life of the law has not been logic; it has been experience" (Holmes); rules whether man-made or not are general and cases are individual. The judge has to "interpret" the law before he can apply it and this means he has to choose between many alternative and equally plausible views of what precedents, verbalised rules of law or statutes amount to. He has to match against competing analogies what he sees as the facts of the case and choose what appears to him the nearest; here his moral and social preferences (of which he may be conscious) and his personal idiosyncrasies or prejudices (of which he may not) are of paramount importance. This, in the author's view, is not finding but making the law. But the basic myth is not merely a false analysis of the judicial function; its practical consequences are evil, for it leads judges to perform in the dark the essential law-making function of the judge and "to sneak into the *corpus juris*" by various boot-legging devices the indispensable moral and pragmatic elements of a just decision which ought to be openly considered and discussed.

Of the strange phenomenon that a hard-headed profession should ignore the patent fact of judicial legislation the author offers a psychological explanation. The law "evokes childish attitudes", "regressive emotions", and lawyers seek for certainty of legal rules as a "father-substitute". Objectors to this thesis will find themselves embarrassed by the vagueness of the argument and the author's device (which he terms a "neglective fiction") of writing as if this were a complete explanation while admitting that it is one among many others (listed in an appendix) for there is no clear statement of why or to what extent it is to be preferred to the others. And the author seems to detect traces of the basic myth and "childlike hankerings" for the father-substitute or of "rule fetishism" in even the modest claim that *sometimes* judges do not make the law but apply it (Pound) or that the law in part consists of rules (Dickinson).

The author's exaggeration of his thesis and his preoccupation with his psychological explanation obscures the genuine difficulty of characterising the reasoning involved in judicial decisions where the analogies supplied by precedents have to be used under changing circumstances. For this purpose the dichotomy between "making" and "finding" the law is about as useful to the legal theorist as the dichotomy between "objective qualities" and "subjective feelings" has been to the moral philosopher.

Ever since the long delayed discovery that logical deduction from true premisses is a very subordinate part of the satisfactory settlement of a legal argument, legal theory like moral theory has oscillated between such twin distorting models. The author in his recoil from the basic myth embraces the theory known as legal realism (which for philosophers might be termed legal phenomenalism) that the law is nothing over and above the actual decisions of judges and the possibility of predicting future decisions. Judges, on this view, always "make" law, never "find" it even in the clearest cases: nothing that happens in a law court could count as finding the law and every statement of law (e.g., "This is a valid will") is either a report of an actual decision or a prediction of one.

But this extreme theory and the accompanying extension of the notion of "making" law to *all* cases has nothing to support it except the truism that no two cases are exactly alike and it obscures as much as the basic myth does the characteristics of legal utterances. Of course only where judicial decisions are made with some uniformity and are predictable with some measure of probability can statements of law ("this is a valid will") be made but this does not show that such statements are predictions of judicial decisions any more than "It is rude not to take your hat off to a lady", or "It is wrong to move your castle diagonally", or "that's bad English", or "Its your duty to volunteer" are predictions of the future behaviour (frowning, disapproval, etc.) of those who subscribe to social etiquette or observe the rules of chess, English, or common morality. The vagueness of legal rules, the interplay between the law and the facts, and the special character of the judge's decision as a criterion of the validity of a statement of law of course differentiate such statements from those just cited but do not render legal phenomenalism plausible.

What is it to apply a rule? At a time when philosophers are inclined to find in "rules" and their "application" the solution to many puzzles it is salutary to consider the difficulties of answering this question even in the legal sphere where these expressions are used in their original and least metaphorical sense: and philosophers may profit from the many illustrations in this book of the reasoning by example which is the substance behind the form of legal argument. In return lawyers might accept from philosophers the suggestion that some gnawing problems will drop away if they cease to ask "What is law?" (a question the author now abjures), and ask instead "What sort of statement is a statement of law?"

H. L. A. HART.

Les Origines de L'Analogie Philosophique dans les Dialogues de Platon.
Par PAUL GRENET. Paris, Boivin, 1948. Pp. 300. Frs. 960.

'L'Analogie philosophique est la représentation des objets situés au delà des limites de l'expérience humaine, par la médiation de concepts empruntés à l'expérience.

'Pour être scientifique et non mythique, elle requiert que le concept expérimental soit débarrassé des caractères qui le proportionnent au monde de l'expérience (première condition); et qu'il reçoive des caractères nouveaux qui le proportionneront au monde métémpirique (deuxième condition).

'De tout cela, Platon s'est rendu compte clairement et distinctement.'

As to the third requirement of 'philosophic analogy', namely 'proportional structure', Plato did not, in Professor Grenet's view, become

explicitly aware of it (still less did he achieve Aquinas' 'analogy of being'); but he did in fact employ it.

A 'mathematical analogy' is a case of $a/b = c/d$, that is, of a quantitative relation between two quantities being repeated in two different cases. If we transfer this from quantitative relations of quantitative magnitudes to other kinds of relation, we might write: $aRb \equiv cRd$, meaning that the same non-quantitative relation R is found both between the pair ab and between the different pair cd . If we then apply our formula $aRb \equiv cRd$ to cases where the pair ab is given in experience but the pair cd is not, we have, I think, what Professor Grenet is calling 'philosophic analogy'. He holds that Plato used it to get knowledge of God, the Forms, and the soul, and believed it the only possible way of getting such knowledge. (He notices and rejects the contrary view that Plato held that our knowledge of the unseen is a kind of intuition or contemplation.) Furthermore, he holds that 'philosophic analogy' can be a valid method of proof as well as a method of suggesting theories. 'Analogy, Resemblance essentially mixed with Difference, is the law both of knowledge and of being and of action.' As Anaxagoras said, 'what appears (in our experience) is a vision of the unseen.'

In 250 large pages of text, clearly written and liberally provided with quotations and references, Professor Grenet sets out this view and also surveys all the ramifications of 'philosophic analogy' in Plato and in previous literature. It leads him into some surprising places; for example, Plato's imitation of his master Socrates appears to be a case of 'philosophic analogy'. And before he is finished Professor Grenet has displayed a great deal of erudition, and illuminated, or at least assembled materials on, several Platonic topics not suggested by his title, so that persons studying any aspect of Plato may be advised to look up their topic in his indexes. His book is of the exhaustive rather than the suggestive kind, and may well be the last book on Platonic analogy for some time.

I cannot think that Professor Grenet's logical assumptions are sound. The immense range which he attributes to 'philosophic analogy' is not the sign of any great power such as the generalizations of mathematics have. It is merely a sign of its triviality. 'Philosophic analogy' is little more than a high name for the fact that we often find two cases of the same relation. Professor Grenet seems to be surprised and relieved to discover that certain analogies in the Hippocratic writings have a proportional structure (p. 253). He does not seem to realize that every complex metaphor or analogy *must* have a proportional structure. As soon as a simple simile $a \equiv b$ is expanded into a complex simile, you inevitably have the proportional structure $a_1Ra_2 \equiv b_1Rb_2$.

Professor Grenet writes as if he had never heard the doctrine that analogy is a good way of getting ideas but a bad way of proving them. His conviction, that 'philosophic analogy' demonstrates, seems to have behind it no better reason than the observation that in *mathematical* analogy, if you are given that $a/b = c/d$, and given the values of a and b and c , you can calculate the value of d . How, in '*philosophic* analogy', we can be given that $aRb \equiv cRd$, and how we can be given the value of the transcendental object c , and how, even given these premisses, we can find a method of metaphysical calculation to infer the value of the metaphysical object d , are three questions that have not occurred to him. He assumes that, because quantitative relations allow calculation, qualitative relations must do so too. But, given that man is to human love as God is to divine love (and what gives it?), how can I calculate anything about

divine love? That Professor Grenet does not understand the method he attributes to Plato is shown by the fact that he says an argument by elimination is a case of it (p. 119, referring to *Rp.* 428A, the proposal to discover justice by elimination). It is also shown by the fact that, in favour of his view that Plato held that the forms are knowable only by analogy, he quotes *Statesman* 285E, which says the contrary: 'Of the greatest and most valuable things there is no image clearly wrought for men.'

Professor Grenet's book has as frontispiece a drawing, by André Ravéreau, of the landscape imagined in Plato's allegory of the Cave. The underground parts are full of interest. But the world outside the cave, which should figure the Forms and the intelligible world, seems to be a featureless desert! Exactly. So far as 'philosophic analogy' can tell us, the unseen world is a featureless desert. Transcendental analogies correspond to the kind of mathematical equation that cannot be solved.

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Les Sophistes. Protagoras, Gorgias, Prodicus, Hippias. By EUGÈNE DUPRÉEL. Neuchâtel: Editions du Griffon, 1949. Pp. 407. Frs.s. 25.

IN 1922 Professor Dupréel of Brussels published *La légende socratique et les sources de Platon*. It claimed that all the chief doctrines of the Socratic dialogues were taken practically unchanged from the fifth century sophists; the ethics were mostly Prodicus', the metaphysics Hippias'. Dupréel's arguments rested largely on parallels in the spurious and doubtful dialogues: the value of these was to be warranted by the references to the sophists in the undisputed dialogues—and *vice versa*. And the arguments were not convincing. Though the reader is not told so, the present work maintains the same thesis with the same arguments. This might be reckoned to account for nearly three quarters of the contents. The remainder consists in reconstructions of Protagoras' and Gorgias' philosophies and an extension of the journey on the hobby horse through Plato's later dialogues. The reconstructions are lucidly expounded and sensible. They might have been more interesting had they taken notice of other people's recent work on the subject: but it would be unjust to pass over their merit. As for the ride . . . Well, as each landmark flashes by—Love, the Right Measure, Collection and Division, Communion of Ideas, Justice, Pleasure—"There's Hippias", we are told, and "there goes the Platonic legend".

The scholar is liable to extra jolts. On p. 131 Dupréel objects (as part of his argument) to Croiset's translation of a Platonic passage, but omits the sentence following, which supports Croiset; on p. 209 the omissions, though marked by dots, make a thorough hash of two theses in the *Dissoi Logoi*; on p. 211 another translation of *Dissoi Logoi* is unpalatable and that of Gorgias, which is supposed to confirm it, wrong.

A. C. LLOYD.

Husserliana, Band I. Cartesianische Meditationen und Pariser Vorträge. By EDMUND HUSSERL. Herausgegeben und eingeleitet von Prof. Dr. S. Strasser. Haag: M. Nijhoff, 1950. Pp. xii 244. 10 guilders.

HUSSERL'S *Méditations Cartésiennes* were reviewed in MIND by Professor A. K. Stout in October, 1932. The present volume is the German text

which Husserl never published, together with the four Sorbonne lectures from which the *Meditationen* were developed. Further unpublished material is to follow in later volumes.

The *Meditationen* cover much the same ground as the *Ideen*. But there are some new things, which are important, and there is perhaps a change of emphasis. The changes are really developments of certain discussions which go back at least to the *Logische Untersuchungen* and which centre in Husserl's conception of "intentionality". Brentano had spoken of the "intentional relation" as the fundamental feature of all forms of consciousness. What he meant was that to be conscious is to be conscious of something. And he classified the "phenomena of consciousness" according to the different ways of having objects, taking the three fundamental classes to be ideas, judgments and interests. Husserl always said that this view of Brentano's was his own starting point. But from the first he took a view of "intentionality" which was radically different from Brentano's and which changed his views not only of perception and consciousness, but of philosophy altogether.

Brentano did not fall into "psychologism", I think. But he held that "psychological analysis", or the analysis of "phenomena of consciousness" was essential in clarifying or understanding the fundamental concepts (such as "truth", "cause", "time", and others) that enter into philosophy. So that psychological analysis was an important part of philosophical method. But there were difficulties in this: difficulties in his notion of "psychical phenomena", and difficulties in his conception of an "analysis" of them. And Husserl developed his own views in trying to deal with them. He held that the intentional relation itself is never something simple. In the *Logische Untersuchungen* he argued that sensation, for instance, has not the intentionality which Brentano said all consciousness has, and that it does not in itself refer to any object. The consciousness of an object, or intentionality, is always what you might call a relation of meaning. Sensation enters into the perception of an object. But the sense datum itself is not the object of perception. What is perceived is the colour of the object, the taste of the object, and so on. An "analysis" of perception would not be an analysis of sensations; it would be something of an entirely different kind.

Husserl calls it an "exposing" of the activities—"Leistungen"—that go into the consciousness of an object. You see something that could be viewed from different sides and in different positions. These would be different views of the same object. And that sort of identity enters into what you mean in speaking of an object at all. But you have given no account of it, and no analysis of it, if you just describe sensory complexes. There is a work of "synthesis" which "constitutes" an object for the mind. And without it there would be no experience. For the object does not come from without. If you speak of what acts on sense organs, for instance, you are already thinking of objects. And no theory of the mind's relation to "things outside" explains how the mind can have objects at all. An explanation of that would have to be prior to all causal explanation, just as it would be prior to all analysis in the ordinary sense. It would still be an exposition of consciousness, but of a "transcendental consciousness" whose work of synthesis or unification makes experience possible. That is why he says you cannot understand ordinary consciousness without reference to a "transcendental" or "pure ego".

In the *Logische Untersuchungen* Husserl at first rejected the view that there must be a pure ego as a centre for experiences. But in the second edition he said that for other reasons there must be a pure ego. And this became central in his later writings. The pure ego appears not as the "pole" of my various inner experiences, but as what makes possible the continuity or connexion and common intelligibility of experience, or of what is experienced. Unless there were this consistency or commensurability of meaning among objects, unless they belonged to a common system of possible experience, you could not think of objects at all. So by their nature as objects they refer to one ego which is the foundation of the possibility of experience. Objects may have an "independent existence", independent of any particular experience. But they must be objects of possible experience; it is only in the possibility of experience—i.e. in the pure ego—that they have meaning as objects at all. ("Being" and "meaning" seem often to coincide; or perhaps being is one kind of meaning.)

Husserl asks whether this view is solipsism, and he discusses the way in which "my pure ego" may have knowledge of "other pure egos", although it does not "constitute" them. He gives more space to these questions here than in his earlier works, and his treatment opens up new topics. Professor Stout has discussed it in his review.

I suppose Husserl's view that knowledge and understanding of existence must be found in knowledge of the self, and his connexion between "meaning" and existence, have been influential. But I think there are confusions in what he says. (His language does not help. It was not only in his views that he departed from Brentano.) He has raised important difficulties. But in his treatment of them he has revived others which darken the discussion. His view of the "constituting of objects" depends on his "method of bracketing" in which he will begin by refusing to assume that there are any objects existing at all. I think that is nonsense, and Husserl ignores what has been said against it. But it shapes the rest of his inquiry. Again, "the transcendental problem", or problem of how the mind can be aware of objects, seems to me as much a confusion in his treatment as it was with those who asked how the mind can know something outside itself. But these difficulties and others rest principally, I think, on his very confusing notion of "Sinnggebung", or giving meaning, which he supposes to be the fundamental feature of any "consciousness". Here again he is concerned with real difficulties. He seems to hold, for instance, that if I said to myself, "I see this", I should be telling myself what I see; and if that is wrong, his whole discussion collapses. But he clouds the genuine difficulties with much else, and that goes through his treatment of other questions.

R. RHEES.

The Foundations of Common Sense. By NATHAN ISAACS. Routledge and Kegan Paul Ltd., 1949. 15s.

THE sub-title of this book is 'A psychological preface to the problems of knowledge'. Mr. Isaacs' contention is that the sterility and inconclusiveness of the discussions by philosophers of these problems are due to their neglect of this preface: it is not so much that they have avoided psychology

as that they have clung to one that is faulty and out-of-date, and above all have neglected to take account of the actual historical processes of getting-to-know whereby first the child and then the man test and re-test by their actions those beliefs about an objective world which are gradually and cumulatively suggested by their experiences. What is wanted, Mr. Isaacs suggests, is a full account of these processes, and psychologists by their disregard for epistemological problems have by default abandoned this most important and *empirical* field of enquiry to be cultivated (fruitlessly) by the non-empirical methods of philosophers. Mr. Isaacs has attempted to make good this deficiency, though he admits that his material is incomplete for want of the necessary research, and in any case this present work is an abridgement of the more detailed study he had written, which he hopes to publish later. In this abridgement he examines in turn the basis in experience of the distinction between truth and falsity, the belief in an objective world, and the belief in causality; and he finds that these foundation-experiences not only causally explain our common-sense beliefs, but also fully support and justify them, so that any purely philosophical attempt to defend or discredit them must end in futility or failure. In these cases the real is the rational, and there is no need to hunt about for reasons to support beliefs which we hold without reason: the close connexions between our beliefs on the one hand, and our perceptions and actions on the other, generally ensure the survival of only those beliefs which pass the examination of experience.

I think Mr. Isaacs is right in suggesting that philosophers have tended to be over-hasty in their rejection of the possible relevance of genetic investigations to many of their standard problems, and right again in suggesting that psychologists have been unduly neglectful of the concrete growth of knowledge in their studies of learning: and of course the two neglects are connected, since what the psychologists fail to provide, the philosophers cannot make use of. But I think also that Mr. Isaacs goes too far in suggesting that genetic enquiries can by themselves provide the complete answer (so far as it *can* be provided) to epistemological questions. This comes out, for example, in his reformulation of the correspondence theory of truth, which he believes to escape the traditional difficulties. He finds the 'experiential basis' of the distinction between truth and falsity in the relation between our expectations of perceptual experiences to come, and those perceptual experiences which actually do come, whether concordantly or conflictingly. This, he maintains, is an authentic relation of correspondence or non-correspondence, but free from philosophical perplexity because it falls wholly within our experience; it is a transcendent relation merely insofar as it is temporal, since the expectation looks forward to a future experience. But 'correspondence of experience with expectation' will hardly do as a complete account of what is meant by 'true', even in its non-logical uses: for, as Isaacs himself points out later on, our beliefs are not limited to beliefs about possible perceptual experiences (obviously not, in the case of beliefs about the past), and it is *this* transcendence (of experience) which philosophers have tried to account for—or deny—in their theories of truth.

Another example is the treatment of the problem of mathematical knowledge, which he recognises to be full of thorns but which nevertheless he is inclined to turn over to the mathematically-trained psychologist for investigation. He himself, in a tentative discussion, suggests that the clue may be found in certain passive properties of objects, such as their

susceptibility to various operations which can be performed without otherwise changing them : e.g. aggregation and separation, inclusion and superimposition. Now whatever one may think of these arguments, surely they are not (in any recognisably-distinctive sense) *psychological* arguments: no psychological laboratory or observatory seems necessary in which to assess them. This apparent inconsistency shows the weakness of Mr. Isaacs' thesis at this point, for to treat the problem of mathematical knowledge exclusively in psychological terms would be rather like treating the Principles of Electricity exclusively in terms of electric shocks.

R. J. SPILSBURY.

A Critique of Logical Positivism. By C. E. M. JOAD. Victor Gollancz Ltd., 1950. 10s. 6d.

DR. JOAD tells us that he decided to write this book, which is mainly a polemic against another book, the first edition of *Language, Truth and Logic*, because of the latter's influence on the thought-framework of his pupils and the 'intellectual fringe' interested in contemporary philosophy. 'I wanted to find out on what precisely this widespread influence was based and to form a judgement as to whether it was merited, hoping that, if my examination showed that the influence was excessive, something might perhaps be effected in the way of diminishing it.'

Actually most of the book is given over to the sort of criticisms which have been appearing in philosophical journals throughout the last fifteen years, some of them plainly formidable, some of them based on misunderstandings inseparable from the compression and misleading appearance of rigour in the parent-work. Joad does not make any claim to originality in his criticisms, and would not probably be put out very much by the charge of misunderstanding, since he could easily maintain that it was no less important to combat these misunderstandings insofar as they had infected the thought of those familiar with Ayer's book and unfamiliar with the many expositions and developments, criticisms and counter-criticisms of it available only to the assiduous culler of magazines not normally purchasable at railway bookstalls.

Nor would Joad think his book rendered superfluous by the vanishingly-small number of fundamentalists among professional philosophers. He is not addressing these primarily. And even if his critique is not distinguished for its scholarship or subtlety, he has at least subjected his chosen target to a much closer cross-fire than anti-metaphysicians have bothered to do with their targets. He has taken the opposition seriously and has not been content to isolate and mock at fragments of doctrine, *dissecta membra* torn from their organic context. He may, it is to be hoped, succeed in stirring a few sleepers from their dogmatic slumbers, so that they no longer identify their imaginative limitations with the limits of the universe.

The least satisfactory chapters are the too-brief discussions of logical constructions and the effects of logical positivism. The former is a confused and confusing caricature of the views discussed, and it is hard to believe that any reader on any 'fringe' will gain any light from it. The latter seeks to show that the effects are likely to be 'the erosion of desirable beliefs' and 'the promotion of undesirable beliefs'. Rather curiously, under the former heading Joad includes, as examples of the break-down of dominant interests and purposes, interest in religion, politics, the service of mankind, ambition, pursuit of wealth, gambling, archaeology and bird-watching—

everything, in short, that can 'give life zest', irrespective of their zest-worthiness. The sincere logical positivist is depicted as one likely to become 'a Bohemian in art, a Laodicean in affairs, a sceptic in philosophy and religion, an inconstant in love and a dilettante in life'. This is necessarily an unsatisfactory chapter, since the only way in which the case against positivists could be made out convincingly would be by a selection from the private and public lives of eminent positivists, showing the inconstancy of Professor X, the Bohemianism of Dr. Y, etc.—a regular *chronique scandaleuse*. In default of this evidence (or a sufficiently bold publisher) Dr. Joad has to fall back on *a priori* concepts and arguments mainly, such as the concept of a 'fund of unexpended credence', the abhorrence of nature for intellectual vacua, and the argument that if a person ceases to accept the rationality of a belief, he ceases to act on it. He omits on this occasion any mention of the field where the charge of timidity and triviality can be most strongly supported—namely the field of actual philosophical enquiry. To quote Joad's frontispiece quotation from Aristotle: 'Although we must consider how we should express ourselves in each particular case, it is still more important to consider what the facts are'.

R. J. SPILSBURY.

Nature and Man. By PAUL WEISS. New York: Henry Holt and Co., 1947. Pp. xxii + 287. \$2.80.

PROFESSOR WEISS has written this book as a philosophical background to a book on ethics, and he tells us in his preface that the two together are to lead to a third volume on politics. He thinks that ethics needs a philosophical background in the sense that what can be said about ethical activity must be a special case of what in a general way, can be said about everything else. "An ethical being ought to be dealt with as illustrating non-ethical principles of universal application." It would be easy to say "Naturalistic Fallacy", but I do not think we should jump in with this so as to foreclose attempts to describe ethical actions in the same terms as overtly non-ethical facts about nature, without examining these attempts on their merits. By "merits" I understand, (a) whether the facts about nature are real or doctored, and (b) whether this way of talking helps us to understand ethical action better.

Professor Weiss thinks that the outstanding attempt to describe ethics in the same general terms as natural processes, while allowing that man, though part of nature, has distinctive powers, was Aristotle's, and he thinks that ethical thinking has largely lived on the capital of Aristotle's attempt. But the Aristotelian outlook assumes that man has never evolved and has fixed capacities. Professor Weiss wants to repeat something like the Aristotelian attempt, while allowing for "nature" being quite different from what Aristotle took it to be. The question is how far the very general descriptive categories he puts forward do in fact help us to understand ethical action. He thinks that we arrive at these general categories by a kind of phenomenological method, but his description of how this works is full of unexplained metaphors, e.g. p. xix, "once we have put our finger on the pulse of the universe to deny which (Sc. the universe?) is to deny one's sanity it is a comparatively simple thing to express the nature of that which we have isolated". His general view is that philosophical truths are those which cannot be denied

without absurdity, the criterion of absurdity in this connexion being that an assertion is absurd if it "contradicts the existence of full-bodied beings, some of whom have minds, dwelling in a full-bodied spatio-temporal world" (p. xviii). But it depends what level you are talking on; this sentence in itself could be read as a philosophical description which everyone would not be prepared to take as the last word.

The particular philosophical truths which Professor Weiss is interested in in this book are concerned with freedom. Ethical freedom is to be seen as a special case of the "freedom which is at the core of every being", for "freedom is a part of whatever is a part of nature" (p. xvi); presumably this means that all natural processes are in some sense free. In what sense? From Professor Weiss' description, freedom seems to mean:

(1) Something like what scholastics would call the contingency of being, the fact that the things that happen happen at all. Professor Weiss calls this their "course", the "march of time" turning possibility into actuality.

(2) Nature is described as made up of a limited number of separate beings, each of which has an "inside" as well as an "outside", the "inside" of anything (what it is in itself) being another sense of its "freedom".

(3) These beings are atomic, self-determining units of activity, aiming at their own good (called their "concern").

(4) Some beings have spontaneity, a power of modifying their actions to conform with their concerns.

(5) Human beings have a power of modifying their concerns so as to aim at an absolute good, which is "the total future as a form of harmony", i.e. the maximum realised goods of everything whatsoever.

As a description of nature, all this is highly speculative. But even if we accept it, how far would it provide us with the basis of ethics? Surely in any case freedom in sense (5) is something very exceptional in nature. Professor Weiss is of course wanting to say both that man is part of nature, and, by giving a non-mechanistic account of nature, say that there are senses in which he can be free. It might be possible to say this, and also to say that man has distinctive ethical capacities. Professor Weiss sometimes writes as if this were all he meant, but sometimes as if ethical freedom, if it is to exist at all, must be an instance of a general kind of freedom found throughout nature. But if ethical freedom has to be an instance of something found throughout nature, we are likely to lose its distinctive character by spreading it as thin as this. Moreover, we may know more about ethical action than we know about the kind of possible foundations for it which Professor Weiss describes. And how far does his description help us to see what ethical action is like? There is, for instance, no attempt to deal with the problems which critics of (broadly) Aristotelian types of ethical theory have seen, such as the cases where the claims of duty and of self-realisation conflict. Professor Weiss tells us (p. 263) that "The maximum good a being has a right to attain is that which would result from the enhancement of itself proportionately to its present value, in consonance with the proportionate enhancement of the rest." What are the proportions? And how do I know that I have enhanced myself enough for the time being and ought to be thinking about enhancing someone else?

DOROTHY EMMET.

Proceedings of the Society for Psychical Research: Vol. XLVIII, Part 176 (April, 1949, pp. 31, 2s. 6d.); Vol. XLIX, Part 177 (Nov., 1949, pp. 52, 3s. 6d.), Part 178 (July, 1950, pp. 54, 3s. 6d.). *Journal of the Society for Psychical Research*, Vol. XXXV, Nos. 656-659 (Jan.-Aug., 1950, 2s each issue). All published by the Society for Psychical Research, London.

I SHALL confine my attention in this review to those papers which seem to raise the kind of questions in which philosophers are most likely to be interested; in so doing I intend no disrespect to those papers which I shall not mention.

It is a common complaint that psychical research has produced plenty of facts but no acceptable integrating theory; and in a quite brief but valuable paper in No. 659 of the *Journal* ("Psychical Research as seen by a Physical Chemist") Dr. P. H. Plesch emphasises the need for such a theory. He suggests that the reason for the ignoring of the results of psychical research by scientists in general is not the inability of the researchers to produce hard facts; the mere establishment of isolated facts which seem irreconcilable with scientific orthodoxy will not be sufficient to induce scientists to modify their theories, for scientific theories "are extremely resistant to discordant observations, and are not overthrown until the advent of a better theory which can assimilate the discordant facts; [and] strange facts which, though not in disagreement with orthodox theory, do not fit into it, are ignored or explained away, until a unifying theory shows their connexion with the old orthodox doctrine". Dr. Plesch complains that psychical research has been "suffering from a surfeit of Bacon" i.e. from the view that "science advances by first collecting facts and then deducing theories from them"; and he urges the need for a "daring and detailed theory" which will guide future experimenting. He has himself some hints to offer of a possible theory in terms of postulates about "*psi* fields", and suggests some experiments which might be designed to test it.

Dr. Thouless' paper, "Experimental Precognition and its Implications", in No. 657 of the *Journal*, is also concerned with the question of an explanatory theory, in connexion with precognition in particular. The feeling that precognition is somehow "impossible" can only be overcome, he holds, by "a change in the language embodying our theory of how things happen. When we have the right way of talking and thinking the difficulty will not be merely explained; it will disappear." He does not claim to know what linguistic changes are necessary, but he does at least direct our attention pointedly to the vital question: What kind of a thing would an explanation of precognition be?

Part 177 of the *Proceedings* contains the Presidential Address by Professor Gardner Murphy, entitled "Psychical Research and Personality". Professor Murphy, as a psychologist, is concerned with the relations between the psychical researcher and the orthodox psychologist and the need for co-operation between them. He points especially to experiments which have been designed to show correlations between ESP scores and the grouping of subjects into psychological types by means e.g. of Rorschach tests; and he pleads for greater use by psychical researchers of the help which may be forthcoming from psychiatry and the social sciences. He suggests that we may come to a better understanding of ESP by thinking of it in terms of psychological needs and psychological barriers, but in addition he

believes that there is a more elusive third factor which he is inclined to think may be interpersonal, *i.e.* lying "in the relations between persons and not in persons as such".

Part 178 of the *Proceedings* contains two elaborate articles which deserve the attention of philosophers, though for very different reasons. The first is "The Experimental Evidence for PK and Precognition", by C. W. K. Mundle. Mr. Mundle points out, not for the first time, that much of the *prima facie* evidence for psycho-kinesis might be interpreted as evidence not for this but for precognitive ESP, and *vice versa*; and his main purpose is to investigate the possibility of experiments which, if successful, would unambiguously differentiate the two. His paper is extremely thorough and ingenious and is impossible to summarise briefly. I shall, however, mention two points: (a) He suggests a revision of the commonsense criterion of the "direction" of a causal relation (*viz.* that this is always from earlier events to later ones) along the following lines: when we have a temporal clustering of a set of events, which are structurally and/or qualitatively similar to each other, round a central event, then we can call the central event the cause of the others, even though some of these occur before the central event. (Is this an example of the kind of linguistic reform which Dr. Thouless has in mind in the paper I have previously referred to?) (b) Mr. Mundle's paper, like so many others, is bound to raise the question, what exactly would experiments of the kind he mentions establish? That PK, *e.g.*, is definitely established as a fact? Yes, but what does this mean? After all, what we are given is a set of correlations between certain series of events plus an account of the circumstances in which these occurred. It makes sense, I think, to speak about a PK- (or precognition-) pattern in these correlated series and circumstances; it also makes sense to describe a certain pattern as ambiguously either psycho-kinetic or precognitive; it is sensible and worth-while to ask what an ambiguously psycho-kinetic or unambiguously precognitive pattern would be like—and Mr. Mundle's ingenuity will certainly help to answer this question. But it is not at all clear what it meant, or what is added, by going on to speak of events as "influenced by PK", "due to precognition", "explained in terms of PK", and so forth. I do not say that *nothing* is added; but it is fatally easy to speak as if the process of deciding by examining the records whether this is PK or ESP were like the way in which we decide by examining the tracks in the mud whether the murderer arrived by car or by bicycle, without realising how misleading this analogy may be.

The other article in this issue is one by Professor Broad entitled "Immanuel Kant and Psychical Research". It deals, mainly in an expository manner, with those of Kant's writings which concern Swedenborg; these consist firstly of a letter written to Charlotte von Knobloch in 1763, and secondly of the *Träume eines Geistersehers*, published in 1766, in which Kant discusses in detail various metaphysical questions which arise in connexion with notions such as that of a disembodied spirit. I shall merely remark that the whole article is, as one would expect, an admirably acute, careful and informative account of a section of Kant's writings with which many of us are quite unfamiliar.

Another paper which may be mentioned here is "Telepathy and Evolutionary Theory" (*Journal*, No. 658), in which Professor A. C. Hardy develops a suggestion he previously made in an address to the British Association about "the way in which our views on the process of evolution might possibly be altered by the acceptance of telepathy as a fact".

It may not perhaps be generally known that the *Journal*, which until recently was printed for private distribution among members of the S.P.R. only, is now (like the *Proceedings*) available to the general public. It is published six times a year.

GEORGE E. HUGHES.

Insight and Outlook: An Inquiry into the Common Foundations of Science, Art and Social Ethics. By ARTHUR KOESTLER. London: Macmillan and Co. Ltd., 1949. Pp. xiv + 442. 25s.

Two main themes dominate this book. One is the tracing of two broad types of tendencies or impulses in human behaviour, which the author calls aggressive or self-assertive, and integrating or self-transcending, respectively. The other is the notion of "bisociation", i.e. the process of regarding an idea or object as belonging simultaneously to two normally uncorrelated fields of thought or behaviour. In terms of these concepts Mr. Koestler gives us an account of the original or creative, as distinct from the merely habitual, aspects of human behaviour. Every creative process, he holds, is bisociative in nature, but these processes vary in kind according to their "emotional charge", in particular to the degree of self-assertiveness or self-transcendence involved in them. This enables a diagrammatic scheme to be constructed. At the one end we have the comic, where self-assertion predominates (buffoonery being the limit of pure or almost pure self-assertion). Half-way along the scale, where self-assertion and self-transcendence are more or less equally blended, we find scientific discovery—what Mr. Koestler, remembering Archimedes, calls the "Eureka process". Where self-transcendence predominates we have the sphere of the serious or emotive arts. At the limit of pure self-transcendence Mr. Koestler places mystical contemplation, though he does not, I think, say enough about this, and what he does say seems to me to be spoiled by an undue stress on the emotional side of mysticism. The three main spheres in which Mr. Koestler works out his theory in detail are humour, scientific discovery and artistic creation, but his fundamental concern is plainly with the larger issues of how one ought to integrate one's life as a whole and of the right ideals for civilised society. The physiological and psychological background or basis of the theory is sketched in fair detail, and there are some interesting criticisms of Freud. A second volume is promised, which is to attempt to put the theory "on a more scholarly foundation".

I have found reading this book an extremely stimulating experience. I confess to a fear, however, that the kind of benefit I have derived from it is not always exactly the kind the author would have wished. I think, e.g., that he imagines he has told me the sober, accurate, universal truth about art in a formula, or a series of formulæ. I don't think he has done this, and I don't think anyone could or ought to try to. (Whether or not Plato was right in talking about the Form of the Beautiful, he never made the mistake of identifying this with a Formula for the Beautiful.) What Mr. Koestler has given us is what I shall call an "illuminating definition" of art. An illuminating definition is one which heightens our appreciation and makes us look for and find important things we had not noticed before; it is characteristic of such definitions that two mutually contradictory ones do not necessarily exclude each other, and that the relevant question to ask about any one of them is not "Can we find any exceptions?" but "Does it enlighten more often and in more important ways than it obscures?" This is the kind of definition of art (if it is to be called a

definition) that we ought to want; as I have said, I think Mr. Koestler has given us a good one, but I think he has succeeded in spite of, and not because of, his conception of what he is trying to do.

As far as Mr. Koestler's handling of the concept of bisociation is concerned, I am full of admiration for its flexibility and ingenuity, of which I cannot hope to convey any impression here. My fear is that it is, if anything, over-flexible: to give one instance, the bisociation involved in artistic illusion (when, e.g., the stage of the Old Vic is seen as the Castle at Elsinore) seems to me less like the bisociation involved in the poetic metaphor than I think Mr. Koestler recognises. This would not be the first time a good idea had been spoiled by being made to do too much work.

It would be easy to pile up the criticisms. I have even heard the book described as "amateur philosophising", and I think I see what is meant: it does seem to me to contain hasty generalisations, tautologies masquerading as factual statements, and an undue love of a technical vocabulary. But I think it would be a grave error to lay too much stress on these faults. For I find that when I have done my worst with the book, the total impression somehow survives all my complaints; and I heartily commend Mr. Koestler's work to those philosophers who are willing to overlook faults of detail for the sake of a stimulating and vivifying discussion of matters of the highest concern.

The style is varied. There are, I fear, dull passages, but Mr. Koestler also knows how to rise well to the aphoristic phrase and the effective metaphor which make a great deal of his writing a pleasure to read.

There are a few misprints. In particular, there is chaos in the spelling of "behaviour" and its derivatives; one likes to know whether one is reading English or American.

GEORGE E. HUGHES.

An Introduction to Metaphysics. By C. H. WHITELEY. London: Methuen and Co., 1950. Pp. xii + 174. 8s. 6d.

'METAPHYSICS', according to Mr. Whiteley, 'has the tasks of speculating on questions not yet soluble, of co-ordinating knowledge, and of criticising assumptions' (p. 11). In showing how metaphysicians do or might go about these tasks, he chooses as a main theme the difference between Materialism and Idealism about 'the status of Mind in the universe' (p. 13). After explaining Materialism, and discussing the extent to which it is able to accommodate facts like the existence of secondary qualities and the influence of mind on matter, he turns to consider the idealist criticism. He explains very clearly, within the limits of space and the probable capacity of his readers, how this criticism has led to the development of Phenomenalism as an *analysis* of the notion of matter. This analysis he regards as largely acceptable, though he thinks it needs to be supplemented by the notion of a non-sensory cause of sense-data, something for which the evidence is to be found in the practical urgency of sensory experience. What remains of idealism is the theory that the universe exhibits superhuman purpose, and about a quarter of the book is devoted to a study of the meaning and value of the arguments for the existence of God, leading to the tentative proposal of three alternative conclusions.

The selection and arrangement of topics are admirably fitted to interest readers unprepared to take seriously 'the fiddling problems of

verbal analysis which now occupy so much of his (the professional philosopher's) attention' (p. 2), and the treatment, which is extremely sensible and workmanlike, will give them a good example of how these interesting and important problems can and should be investigated. Traditionalists will miss a discussion, by more than implication, of the classical problems of free will and immortality, and iconoclasts may feel that if, as is admitted on p. 165, it is misleading to state the main problem as if Mind and Matter were two distinct things or substances it would have been advisable to state it in some other way. Nevertheless, as an introduction to current metaphysical thinking (which cannot be expected to become post-Rylean at a bound), Mr. Whiteley's book would be hard to better. The general reader and the elementary student (and his instructor) are much in Mr. Whiteley's debt. There are an index, a useful glossary and helpful suggestions for further reading.

D. R. COUSIN.

The Concept of Deity. By E. O. JAMES, D.Litt., Ph.D., D.D., F.S.A. Hutchinson's University Library, Senior Series, 1950. Pp. xiii + 195. 18s.

THIS is a very different sort of book from Professor Ryle's *The Concept of Mind*. Professor James writes primarily as an anthropologist concerned to give an historical and comparative account of the idea of God. It is true that he considers the philosophical implications of theism as falling within the compass of his inquiry and that his three final chapters are given over mainly to questions of significance and validity, but it is clear that his dominant interest is not philosophical and that he is not familiar, for example with the philosophical method exemplified in *The Concept of Mind*. This much is evident from his cursory discussion of Logical Positivism (p. 143).

To this extent the philosophical reader is likely to be disappointed and the reviewer who looks for solutions to his own philosophical perplexities may seem ungrateful for this learned survey of the facts which engender them. A purist might argue that such a book would have done better to confine itself to the strictly historical and anthropological aspects of the subject. But here Professor James is surely right. It is impossible to describe and interpret a whole range of human beliefs without to some extent evaluating them; and it is best that this should be done consciously and critically.

What, then, might one reasonably expect of a book like this, over and above the historical survey? The following suggest themselves:

(1) Some indication of the function of the 'concept of deity' in human societies—a matter for the anthropologist.

(2) Some discussion of what it is that the concept denotes. Does God exist? And since conceptions of His nature have differed, which is the true conception?

(3) Some consideration of the criteria for deciding questions of type (2). How are we to determine whether God exists, or decide between different accounts of His nature? In so deciding are we deciding a matter of fact, or adopting a policy or neither of these?

How Professor James answers such questions as these is not immediately clear, because he is not generally very explicit about the questions he asks and tends to slur over the distinctions between them. His answers

have therefore to be elicited by the reader—a task in which he is not assisted by an excessively abstract and summary style of writing and a marked tendency to turn, whenever possible, from the Philosophy of Religion to the History of the Philosophy of Religion. Both characteristics are exhibited in Ch. VII on 'The Philosophy of Theism' in the course of which one is conducted from Plato to Whitehead in twelve pages so condensed as to be almost unintelligible.

Professor James admits that we cannot prove the existence of God deductively or experimentally. But in this respect, he claims, our knowledge of God is like our knowledge of other people. 'Our knowledge of the one as of the other', he quotes Professor de Burgh, 'is based on the experience of their presence'. He does not develop this, but goes on to draw an analogy between our knowledge of God and of the causal principle or the hypotheses of mathematical physics, both of which take us beyond direct observation and are not susceptible of absolute proof. 'Thus', he concludes in a sentence which exhibits his style at its worst, 'Epistemological arguments for the existence of God cannot be expected to establish more than a justification for the use of the term "Deity" to explain certain features of spatial abstractives which are not self-explanatory within human experience' (p. 151).

This, as he rightly recognises, will not do for the practical purposes of religion: 'In the last analysis the test of all theoretical reasoning on the subject is whether it produces a God who is adequate to the demands made on Him by His worshippers, both for their own inmost needs and for the ultimate purposes of the universe at large'. He notes that the interpretation of God that 'has made the most ready appeal' is that of 'a supra-mundane providence at once above and within the world He sustains. . . . To fulfil the essential conditions of religion, it must be the affirmation of a supernatural Reality responsive to human needs, the objective ground and sovereign moral ruler of the universe, the personal source and conservator of value, the ultimate standard of conduct, a worthy object of worship, the guide of man from the cradle to the grave and the hope of immortality.' This idea occurs in most systems. Sometimes one or other element is 'lacking or given undue emphasis, thereby modifying the efficacy of the concept as the religious interpretation of ultimate reality' (p. 152). The same point is frequently made: one more quotation will suffice. 'Neither an absentee deity nor an atheistical determinism, however, meets the deepest needs of struggling humanity as its strength and stay in the mastery of its environment and in the solution of the manifold problems of everyday life and experience' (p. 164).

Here the test proposed for 'all theoretical reasoning on the subject' is a pragmatic one. The efficacy of the concept as 'the religious interpretation of ultimate reality' depends on its being interpreted in a way that meets the deepest needs of humanity. It is natural to ask what is the theoretical reasoning for which this pragmatic test is proposed? Not surely the search for a *definition* of religion. This could scarcely comprise 'all theoretical reasoning on the subject'. It looks as if it is offered as a means of deciding between different conceptions of God's nature and perhaps even as a proof of His existence—though not, of course, a deductive or experimental one. If so, the straightforward objection to this procedure is that it begs both questions, unless we have independent grounds for supposing that the deepest demands of our nature are to be met.

No doubt—if he had said nothing else—Professor James's way of speaking here might be held to support an interpretation of the nature of

religious belief in terms of which this objection would lose its point. According to this view to say that God was 'real' or 'existed' would be to say that belief in Him enabled men to satisfy their deepest needs, master their environment, solve the problems of everyday life and so on. This is what would be meant by 'the religious interpretation of ultimate reality' (as distinct, *e.g.* from the scientific interpretation). This would have the advantage for the theist in his present predicament of relieving him of the burden of asserting a matter of fact. And this could be made to assort well with the anthropological interpretation of religion which Professor James is up to a point inclined to accept: that the function of religion is to reassure men and help them to cope with their environment.

However, this is not, I think, what Professor James intends and it would certainly not satisfy most Christians. What he has, in effect, done, is to take as his standard the Christian conception of God and test other conceptions by it. He does in fact believe that this conception of God meets human needs better than any other, but for his grounds for holding that this is the *true* conception we must turn to his chapter on Divine Revelation (Ch. IX).

Most religions, he points out, appeal to a revelation—usually in the form of propositions. The Christian Revelation takes the shape of events, which it is the business of the theologian to interpret. Hence Christian theology begins as history and must use historical canons of evidence where these apply. Indeed he goes so far as to suggest that the method of the modern theologian is inductive: 'Moreover, the former deductive method of interpretation based on revelational presuppositions has been abandoned very widely in favour of an inductive scientific method in which facts are carefully attested and verified by the aid of all the available evidence as in any other department of empirical knowledge and doctrines formulated like empirical hypotheses as conclusions drawn from the data' (p. xi).

Thus the test of a Christian doctrine is whether it offers the most satisfactory interpretation of certain historical events. 'Inductive' is presumably metaphorical in this connexion, if it is not to mean simply 'scholarly'. For clearly no ordinary inductive method could establish, for example, that Jesus Christ was the Incarnate Son of God. The closest analogy would be with historical method, but there is an obvious difference between 'He was incarnate' and 'He was crucified under Pontius Pilate'. Professor James makes some attempt to illustrate this method by applying it to selected doctrines—the Trinity, the Virgin Birth, and the Immaculate Conception of the Virgin. The first he considers proved, and the second arguable. The last shows 'how readily interpretation passes into speculation, legend falsifies history and hallucination is made the medium of divinely guaranteed theological propositions and formulated beliefs, usually appealing to the emotions at the expense of the reason, regardless of the nature of the evidence on which they are based' (p. 192). Revelation is to be sought in 'divinely ordered events verified at the bar of history, reason and spiritual experience'.

There is much here that is suggestive, but it is presented in too condensed and summary a fashion to be of much use. It is a pity that Professor James did not expand and explain some of these statements and illustrate them more fully.

BASIL MITCHELL.

Man for Himself. An Enquiry into the Psychology of Ethics. By ERICH FROMM. Routledge and Kegan Paul, 1949. 12s. 6d.

THE author of this Neo-Freudian approach to ethics is well known for his *Fear of Freedom* in which he outlined ways of escape from the psychological predicament which beset man after the breakdown of the Feudal system and the development of capitalism and individualism. His thesis was that freedom from various constraints was achieved at the cost of a widespread feeling of aloneness in the face of uncontrollable forces and of insecurity in a society whose close-knit structure had been shattered. Several ways of trying to fill this void—e.g. the recipes of Luther and Calvin, subservience to a Leader or to public opinion, mystical absorption, and so on—he regarded as psychological lapses; the only way forward from individualism was along the road of “positive freedom”, which must supplement negative freedom from the old constraints. *Man for Himself* is a detailed attempt to work out what Fromm meant by “positive freedom” in his earlier book and what the philosophers have traditionally called the good life.

The theme of the new book is reminiscent of Bishop Butler. Man has a “primary potentiality” for goodness, or the life of “productiveness”; vice is self-mutilation. The old doctrine of function, as taught by Aristotle, Spinoza and others is revived in a more modern garb. Fromm’s quarrel with his philosophical forerunners is only that they lacked the psychological knowledge necessary to fill in the details of humanistic ethics; his quarrel with modern psychologists is that they have encouraged ethical relativism by their concentration on the irrational sources of conduct. The old philosophers were concerned with the good life for man as such and said a lot of illuminating things about it; modern psychologists tell us only about the causes of maladjustment within a given system of ethical norms without raising the question whether certain types of socially approved character (e.g. the “marketing” type) are “socially patterned defects” which are hindrances to productive living. Psychologists can and should concern themselves with the good life; they can and should provide a system of norms which are objectively valid. Ethics is an applied science concerned with the art of living ancillary to the theoretical science of psychology.

Much of the book is an interesting and colourful psychological description of the type familiar to readers of existentialist rather than Freudian literature. Fromm thinks that there is a universal human nature which derives partly from man’s innate physiological equipment and partly from his earthly predicament. Man’s imagination, reason, and self-awareness are important as his differentiae in so far as they lead to the universal predicament of aloneness, of fear of death, and consciousness of separation from other people and the processes of nature. Man works out all sorts of “frames of orientation and devotion” in order to re-establish equilibrium between himself and the rest of nature. Religious thought and practice is one attempt to do this, though most religions exemplify immature and pre-genital attempts to cope with the situation. The solution lies in “productiveness” or a way of behaving characteristic of Freud’s ill-defined genital character.

To describe a man as virtuous or vicious is to make a remark about his character and not about his temperament. Fromm claims that the failure to insist on this distinction is one of the roots of ethical relativism, whose

exponents regard ways of life as a matter of temperament. Temperament is an *innate* mode of reaction; character is a system of tendencies acquired in dealing with man's "existential dichotomies". The ethical question is not whether one should be cyclothymic like Goering or schizothymic like Himmler in one's destructiveness; rather it is whether one should be destructive at all, and, if so, towards what. The vicious characters, in Fromm's sense of "self-mutilating", are the receptive, exploitative, hoarding, and marketing orientations. The first three, which correspond roughly to Freud's oral sucking, oral biting, and anal characters are to be found in all societies; though receptiveness will be predominant in an authoritarian society, exploitativeness in a piratical and early capitalist society, and hoarding in a middle class after the rise of capitalism. The marketing type, however, who is what others want him to be, a player of many parts who sells his personality like a commodity, is a product of modern industrialism and commercialism, a recent development in the history of vice. In contrast to these vicious orientations is the virtuous or "productive" character who "comprehends the world" through reason and love, and whose attitude to people and things is characterised by care, responsibility, respect, and knowledge. He is the man who realises the potentialities inherent in him, whose activity originates from spontaneity guided by rational decisions on the basis of experience rather than from outside authority or internal compulsions. His conscience is not the internalised voice of authority convincing him of his guilt and unworthiness. Rather he has a humanistic conscience which is a "reaction of our total personality" to its proper functioning and the "voice of loving care for ourselves". The productive man loves himself as well as others, knows what his long-term interests are, and has the integrity to pursue them.

What is to be said about this book? The philosopher's first reaction must surely be to point out the need for Fromm to clarify the relationship of his norms to the facts which are quoted as being relevant to them. Are the norms of "productiveness" counsels of prudence based upon the study of the sorts of character who break down within a society or the sort of "socially patterned defect" that seems to cause widespread unhappiness in a certain culture? And what of this "primary potentiality" for goodness? This seems as vague and indeterminate as the humanistic conscience which is the voice of the "total personality" (including the receptive, exploitative, and hoarding tendencies?). Similarly philosophers would feel unhappy about the doctrine of function being revived together with a universal human nature. Certainly one of the key problems in the nature and convention controversy is to decide what are the invariable and unalterable dispositions of man. Fromm touches the heart of the matter here. But is his hypothesis about the universal predicament of aloneness convincing—especially in view of his own important contribution on the connexion between psychological predicament and the social and economic conditions of the post-Medieval period?

Many more criticisms of a similar kind can be levelled at this recent attempt to justify the ethics of humanism. But one still feels that philosophers who have talked about the good life and Fromm who talks about productive living are talking about a way of behaving which can be practised under a variety of cultural and climatic conditions with similar psychological consequences to the individual. It is hard to believe that temperamental preference alone is responsible for singling out activities and attitudes like creativeness, spontaneity, respect for facts and people,

co-operation, and so on, as characterising the good life. Fromm sees, in his insistence on the importance of distinguishing temperament from character, that it is insufficient to plead in reply to the humanist that these activities are "not my cup of tea" or that one must "make a decision" about this sort of thing. But his failure to discuss the logical status of his norms leaves untouched the crucial philosophical problems which his book raises.

RICHARD PETERS.

Social Psychology (Third Edition). By LA PIERRE and FARNSWORTH. McGraw Hill Book Co., 1949.

THIS is a text book designed, I imagine, for the masses of students in U.S. universities who find themselves taking undergraduate courses in "Social Psychology". Its chief interest to an adult audience is the attitude it reveals to this subject.

The authors point out that historically the psychologist has been interested in the nature of the individual human being, the sociologist in the social organisation of men. Together they left out a third question, viz. "the relation between the psychologist's 'individual' and the sociologist's 'society'". "This", they say, "is the general problem with which the social psychologist is concerned." They attack this by contrasting various historical views with what they call the contemporary view about why men behave as they do in communities. Historically, this behaviour has been said to be due to human nature, the search for pleasure, God, instincts, imitation, and so forth. The contemporary view replaces the whole idea of a one-way cause and effect by the notion of "interactionism"—men behave as they do largely because of their interactions with other men. Consequently, it becomes essential to investigate the way in which the human animal becomes a social human being. This process of "socialisation"—to use the standard term—produces a unique result, viz. the "personality" of the individual. For the purposes of analysis, the authors split up the attributes of personality into four types. (1) Those involved in making the individual treat a situation or other person as "a such and such" or "a so and so". They call this phenomenon "personality stereotyping". (2) "Normative attributes". These exhibit the norms of the group, and represent the successes of socialisation into the group. (3) "Deviant attributes". These are peculiar to the individual and represent failures in socialisation. (4) Attributes that lead him to control his deviant attributes and overtly conform. But social disorganisation and malpreparation can upset, in various ways, the process of socialisation, and produce socially atypical and psychologically abnormal modes of adjustment. These are classified and described. The last section consists of listing and describing various social situations where the socialised human being plays his role.

What is the value of all this? The book is sober and well-balanced, if rather dull. It carefully covers the field—or most of it—and provides a bibliography and author index of 70 pages long. It offers a set of verbal pigeon holes by means of which the bewildered student (and perhaps also the instructor!) can try to order the chaotic mass of empirical material at his disposal. It is quite right to emphasise that there is a vast range of questions that neither traditional psychology nor contemporary sociology set out to answer; and it is in these that the social psychologist is interested.

On the other hand, I doubt whether the pigeon holes provided are of much heuristic value. I doubt whether they are as up to date as the authors would like to believe. For example, we do not want to be told that "socialisation" is important. We want to know in detail just how this works. But the authors cannot satisfy us because they do not state and discuss the puzzles that face workers like Kardiner. One of the reasons for this is (shall we say?) their prejudice against the clinician and clinical methods. Then, in spite of the intentions of the authors, the book gives the impression of mistaking verbal for empirical discoveries—perhaps a characteristic sin of American writers. This distracts attention from the sort of empirical work that has been done and that still requires to be done. One certainly does not obtain from this book any sense of the theoretical and practical importance of psycho-social questions, and of the pivotal position they occupy in the social studies. I refer, for example, to questions like the psychological "basis" of the State; the ways in which different psycho-social environments for the child produce different personality types; the gains and stresses of different cultural arrangements; the interpersonal factors contributing to economic efficiency; the psycho-social consequences of different methods of dealing with philosophical conflicts.

Moreover, this book encourages one to treat Social Psychology as a demarcated subject with concepts of its own; and this leads us to ignore the important discovery in recent years that it is by co-operation between workers in different disciplines (e.g. sociology, psychology, anthropology, psychiatry) that the complexity of behaviour in groups will best be understood. It is unhelpful, therefore, to suggest that the gap between traditional psychology and sociology is dealt with by "a subject" called "social psychology", with its own boundaries and concepts. One consequence of this unhelpful suggestion is to perpetuate the mistaken idea in university circles that the social sciences are separate subjects, like physics and biology. I doubt whether they are all like this. But because of this mistaken idea universities go on demanding that any set of social enquiries shall become, or be frozen into, "a subject" before it can be studied or taught. Then, since psycho-social enquiries are so patently not a subject at all, no move is made to incorporate these enquiries within the framework of university organisation and teaching. This, I think, is one of the minor reasons why the most active centre in this country for the study of social psychology has no university connexion. I refer to the Tavistock Institute of Human Relations. It is, too, one of the minor reasons why university organisations expressly designed to pursue social studies (like, e.g., the London School of Economics or the Board of Social Studies at Oxford) are not pursuing psycho-social enquiries with anything like the vigour or attention they deserve.

B. A. FARRELL.

Le Rationalisme de J.-J. Rousseau. By ROBERT DERATHÉ. Paris: Presses Universitaires de France, 1948. Pp. 201. 300 frs.

"ROUSSEAU est-il rationaliste?" M. Derathé explains in the Introduction how this question infallibly embarrasses all serious students of Rousseau's works. For at times he seems to be an emotionalist and at times a rationalist. His interpreters have divided into two camps: one group have tried to show that in spite of several rationalist formulae

his doctrine, like his method, is emotionalist; their opponents have claimed that the appeal to emotion does not prevent Rousseau from remaining fundamentally rationalist. M. Derathé discusses the problems in four chapters: 'The development of reason in man'; 'Reason and religion'; 'Reason and conscience'; 'Refutations of *Emile* in the eighteenth century'. His conclusion is that in Rousseau's system reason is a healthy faculty (The opposite of 'healthy' in this context is 'fallen'). The final pages of the book explain 'in what sense Rousseau takes his place in the rationalist current, in spite of his appeal to emotion'. There is also an Appendix on Ernst Cassirer's neo-Kantian interpretations and a four-page Bibliography.

This is, as far as it is possible for a reviewer who is not a Rousseau scholar to judge, a painstaking, thorough, and convincing study. And yet philosophic readers in England may well be left thoroughly dissatisfied. For though this book was in fact first published in 1948 it might almost equally well have appeared in 1848, or even earlier. M. Derathé—like most if not all the historians of philosophy—seems to have been utterly untouched by what—for want of a better name—may be called modern linguistic developments. This isolation is probably no-one's fault—Or is it perhaps Hitler's?—but it does enormously reduce the value for English readers of studies such as this.

M. Derathé, for instance, invariably writes as though the subject of discussion were the capacities and proper functions of various things or agencies called reason, conscience, will, emotion, and so forth. He claims that Rousseau has "an entirely new theory which submits reason to the direction of conscience" (p. 73), and that "reason by itself has no effect on our will: emotion must intervene to enable us to conform our conduct to the rules of reason" (p. 138). Throughout, phrases like "condemning reason" (p. 43), "consulting his reason" (p. 16), and "the collaboration between emotion and reason" (p. 7) constantly recur. Such personifications do have a place in elegant prose. But it is surely important never to forget that there are not really any such people as Reason and Will. Talk of consultation and collaboration between them, of their submitting to or controlling one another is all mythological. Unfortunately M. Derathé takes the myths so seriously that he never writes unmythologically at all, never cashes the mythological counters into common currency. It is, therefore, never clear whether what is being done is psychology, morals, or logical analysis, nor yet what concretely and precisely is being said about whichever subject it is which is being discussed at any particular time.

These faults perhaps were Rousseau's faults. But they do not therefore have to be the faults of his interpreters also. Lean men may drive fat oxen.

ANTONY FLEW.

The Theory of Probability. By HANS REICHENBACH. University of California Press. Pp. xvi + 492. \$12.50.

REICHENBACH's *Wahrscheinlichkeitslehre* was published in 1935. A critical notice by E. Nagel appeared in *MIND* in 1936, and Reichenbach's views are now widely known. In so far as this book is a translation, by Ernest H. Hutten and Maria Reichenbach, of the German work, incorporating no important changes of view, it is not necessary to give a general account of its contents. But there has been a considerable

recasting of parts of the book, thoroughly justifying the description of the book as a second edition and not a mere translation; it would be as well to give some indication of these changes. In Chapters I-VIII, in which the more formal logical and mathematical parts of the theory are given, the changes are relatively unimportant, being mainly slight and solely in the interests of clarity and accuracy. But the original chapters IX and X, in which the application of the formal theory and its relation to the general problem of induction were discussed, have been entirely remodelled in three new chapters. A good deal which in the original edition was sketchy and programmatic is here argued with much greater elaboration. It becomes a good deal clearer here than in the original how Reichenbach defends his thesis that the frequency theory of probability can cover the probability of single cases and hypotheses; the further thesis that all induction, including probability statements, is ultimately based on simple enumeration, and that it is justified as the best available method of going beyond the data, are also much more thoroughly worked out. There can be no doubt that as a result of these changes we have a much more valuable book than the original, especially for those whose interest is more philosophical than mathematical.

The translation is on the whole good and idiomatic, even if it betrays a German origin by a copious use of abstract nouns ("equal causes are always followed by equal effects" (p. 470) is an unusual lapse). The book is very well produced, which one would expect at the price demanded.

The presentation remains, however, very formal. Most of the argument regarding the probability of hypotheses, for example, rests on the assumption (p. 434) that scientific laws can be interpreted extensionally as general implications. This will not satisfy everybody. One may also feel that Reichenbach at best shows that probability statements which are not overtly numerical *could* be interpreted as frequency statements, rather than that they are in fact used in this way.

J. O. URMSON.

La logique de l'assertion pure. By JEAN DE LA HARPE. Presses Universitaires de France, 1950. Pp. xii + 80, and end-paper table. 200 fr.

M. DE LA HARPE has produced the outlines of a calculus, mostly in the form of tabulated equivalences, whose principal innovation is the use of the two symbols A and E interpreted as "il est établi" and "il est exclu". The first chapter, supported by references to Lalande's *Vocabulaire Philosophique*, distinguishes between the assertion of a proposition and its content (*lexis*). The assertion is originally defined as a kind of mental act, but is later treated as a formal operation on a proposition. Thus the symbol A denotes the assertion and consequently AP denotes the asserted proposition P. M. de la Harpe notes that in this case "l'assertion et la lexis ne font qu'un; elles se confondent", but he goes on to produce expressions in which the symbols A and E are iterated. AAP is then interpreted "il est établi que P est établi". It seems clear that this is not a logic of assertion. The distinction between assertion and content is useful in many contexts, but if there is a logic of pure assertion, it is not as M. de la Harpe claims it is "une logique opératoire" in this sense. One can say the same thing twice over, but this is not to perform two successive operations on the content of a proposition. The rest of the book is concerned with symbolic versions of various everyday

expressions which are felt to be inadequately rendered by the propositional calculus. These include hypothetical, problematic and modal judgments as well as the temporal senses of "and" and "then". I do not feel certain that these expedients are entirely successful since the rendering "il est établi" for A seems more and more difficult. Indeed I am doubtful whether the symbol A can have any consistent interpretation throughout the book.

O. P. WOOD.

Histoire et Sciences Politiques. By STEN SPARRE NILSON. Chr. Michelsens Institut. Bergen: J. G. Boktrykkeri, 1950. Pp. 162.

THE author's first object is to examine the extent to which political scientists (as distinct from economists) have been able to produce causal explanations in terms of quantitative correlations in their data. He deals mainly with the uses which have been made in this connection of the electoral statistics of Western Europe and the Anglo-Saxon countries during the last century. His discussion of public opinion surveys is only summary and concerned largely with the ways in which they can aid the interpretations of electoral statistics. And he only devotes three lines to the "quantitative semantics" of Professor H. D. Lasswell. The author's conclusion is that the application of quantitative techniques may shed some light on the relative importance of different causal factors in modern politics, but, like Aristotle, he believes that political scientists should not seek more precision in their generalisations than their subject-matter admits.

The second part of the book discusses the wider question whether quantitative techniques are of any use for the discovery of general laws from the study of history. The discussion centres round two main problems: is it possible to assess quantitatively the causal importance of the parts played in history, firstly, by prominent individuals, and secondly, by the means of production? The author's conclusion is that these techniques are much less useful here than in the more limited field of electoral politics. He may well be right in this though his examination of concepts like "historical cause" or "historical law" is rather cursory. In an appendix he argues that the international balance of power is incapable of a quantitative interpretation. He dismisses (p. 132) L. F. Richardson's attempt to produce one (*British Journal of Psychology*, Monograph Supplement XXIII) on the grounds that it is based on a false assumption of international free trade, without discussing the procedural merits of Richardson's mathematical equations for the expression of the international interaction of threats, co-operation, grievances and costs.

The book contains a useful bibliography, and affords a readable conspectus (in French) of the work so far done in this field, in which Americans are well ahead of everybody else.

JONATHAN COHEN.

Two Memoirs. By JOHN MAYNARD KEYNES. Introduced by DAVID GARNETT. London: Rupert Hart-Davis Ltd., 1949. Pp. 106.

I RECOMMEND the second memoir in this book to all lovers of the work or personality of G. E. Moore. *Principia Ethica* was published at the end of Keynes' first year at Cambridge, and became, together with its

author's personal presence, one of the strongest influences upon Keynes and his friends. In this intimate memoir Keynes gives a very delightful and illuminating picture of how this book affected him and his circle as undergraduates; it is astonishing but convincing to read that the effect was far more "aesthetic", in the manner of Pater, than "moral" in the manner of the ethical works which we think of as its successors. Keynes also expresses his judgments on the book in later years; and here too he is quite unexpected and very stimulating. And there are fascinating glimpses of Cambridge personalities, among which the most charming is the description of Moore's mastery of "the accents of infallibility". Yes, the twenty-three pages of the second memoir in this little book, called "My Early Beliefs", are a first-class delight for everyone who has enjoyed the conversation or the writings of that unique and well-beloved philosopher, G. E. Moore.

RICHARD ROBINSON.

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IX.—NOTES

TRINITY COLLEGE, CAMBRIDGE

PERROTT STUDENTSHIP IN PSYCHICAL RESEARCH

The Electors to the Perrott Studentship are prepared to receive applications from candidates.

Psychical Research is defined, for the purpose of the Studentship, as 'the investigation of mental or physical phenomena which seem *prima facie* to suggest (a) the existence of supernormal powers of cognition or action in human beings in their present life, or (b) the persistence of the human mind after bodily death'.

The Studentship is open to any person who shall have completed his or her twenty-first year at the time when the election takes place. A Student may be re-elected once, but not more than once.

The Studentship is tenable for one year, and the Student will be required to devote a substantial part of the period of his tenure to investigating, in consultation with a Supervisor to be appointed by the Electors, some problem in Psychical Research. The Student shall not, during his tenure of the Studentship, engage in any other occupation to such an extent as would in the opinion of the Electors interfere with his course of research. Residence in Cambridge is not required.

The Studentship will be of such value, not exceeding £300, as the Electors may award after considering the nature of the research which the candidate proposes to undertake.

Applications from candidates should be sent to *The Secretary, Perrott Studentship Electors, Trinity College, Cambridge*, not later than 30th April, 1951. Intending candidates should write to the Secretary for further details before applying.

The election to the Studentship will take place in the Easter term of 1951, and, if a candidate be elected, his tenure will begin at Michaelmas following the election.

JOINT SESSION OF THE ARISTOTELIAN SOCIETY AND THE MIND ASSOCIATION AT EDINBURGH UNIVERSITY

July 6th to 8th, 1951

Programme

FRIDAY, JULY 6th

At 8 p.m. Chairman: Mr. John Wisdom. Address by Professor J. MacMurray. "Concerning the History of Philosophy."

SATURDAY, JULY 7th

At 9.45 a.m. Annual General Meeting of the Mind Association.

At 10 a.m. Symposium. "Thinking and Language." Miss Iris Murdoch; Mr. A. C. Lloyd; Professor Gilbert Ryle.

At 8 p.m. Symposium. "The Logical Status of Supposition." Mr. David Pears; Mr. A. H. Basson; Mr. B. Mayo.

SUNDAY, July 8th

At 10 a.m. Symposium. "On what there is." Mr. P. T. Geach; Professor A. J. Ayer; Professor W. V. Quine.

At 8 p.m. Symposium. "The Freedom of the Will." Mr. Stuart Hampshire; Professor W. G. MacLagan; Mr. R. M. Hare."